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ABSTRACT

This study reviews the evidence on the impact that manpower training programs have had on the earnings of the poor, in order to assess the likely success that greatly expanded training programs would have in reducing the amount of public assistance payments and the size of welfare roles. Between 1963 and 1971, the Federal Government obligated 6.8 billion dollars for training 6.1 million people. This study examines five of these programs: Manpower Development and Training Act (MDTA), Neighborhood Youth Corps (NYC), Job Corps, Job Opportunities in the Business Section (JOBS), and the Work Incentive Program (WIN). MDTA and NYC are the largest of the manpower training programs, and have been in operation the longest. MDTA offers a wide range of institutional and on-the-job instruction; it serves disadvantaged persons, although not exclusively. WIN treats only welfare recipients. NYC, JOBS, and Job Corps deal exclusively with disadvantaged persons, though each has its own structure and training methodology. JOBS is a private venture which is federally funded. The majority of Job Corps centers train youths at sites away from their home environment. NYC provides work experience, earnings, and training to high school students and dropouts, and encourages them to continue their education. (Author/JM)

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STUDIES IN PUBLIC WELFARE

PAPER NO. 3

THE EFFECTIVENESS OF MANPOWER TRAINING PROGRAMS: A REVIEW OF RESEARCH ON THE IMPACT ON THE POOR

A STAFF STUDY

PREPARED FOR THE USE OF THE

SUBCOMMITTEE ON FISCAL POLICY

OF THE

JOINT ECONOMIC COMMITTEE
CONGRESS OF THE UNITED STATES

NOVEMBER 20, 1972

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(II)

LETTERS OF TRANSMITTAL

NOVEMBER 16, 1972.

To the Members of the Joint Economic Committee:

Transmitted herewith is a staff study entitled "The Effectiveness of Manpower Training Programs: A Review of Research on the Impact on the Poor," by Jon H. Goldstein of the subcommittee staff. This is the third in a series of studies being prepared for the use of the Subcommittee on Fiscal Policy in connection with a comprehensive study of this Nation's welfare-related programs under the general title of Studies in Public Welfare. This study reviews the evidence on the impact that manpower training programs have had on the earnings of the poor, and assesses the likelihood that greatly expanded training programs will reduce the incidence of poverty and the size of the welfare population.

Robert I. Lerman of the subcommittee staff contributed valuable comments at every stage of the research. The views expressed in this paper are exclusively those of the author and do not necessarily represent the views of the Subcommittee on Fiscal Policy, the Joint Economic Committee, individual members thereof, or its staff.

WILLIAM PROXMIRE,
Chairman, Joint Economic Committee.

NOVEMBER 13, 1972.

Hon. WILLIAM PROXMIRE,
Chairman, Joint Economic Committee,
U.S. Congress, Washington, D.C.

DEAR MR. CHAIRMAN: Transmitted herewith is a staff study entitled "The Effectiveness of Manpower Training Programs: A Review of Research on the Impact on the Poor." This is the third of a number of such study papers being prepared to forward the work of the Subcommittee on Fiscal Policy in its objective and nonpartisan review of all phases of the Nation's system of welfare-related programs. The studies will be published in a series under the general title of Studies in Public Welfare.

This study reviews the evidence on the impact that manpower training programs have had on the earnings of the poor, in order to assess the likely success that greatly expanded training programs would have in reducing the amount of public assistance payments and the size of the welfare roles. Between 1963 and 1971 the Federal Government obligated $6.8 billion for training 6.1 million people. This study examines five of these programs: Manpower Development and Training Act (MDTA), Neighborhood Youth Corps (NYC), Job Corps, Job Opportunities in the Business Sector (JOBS), and the Work Incentive Program (WIN).

One major conclusion can be drawn: Manpower programs are not a substitute for income supplement programs. Training does increase

the earnings of the poor and reduce the poverty gap, but continued income supplementation is likely to be necessary for the average trainee. Even those studies with the most optimistic results estimate average posttraining annual earnings levels well below the poverty line. For example, in a recent sample MDTA trainees averaged $3,100 in posttraining annual earnings, over $800 below their poverty line.

The impact of training varies with the characteristics of the individual trainee and the existing economic conditions. Continued high levels of unemployment in the economy will make it impossible for trainees to realize the full benefits of training. Earnings increases are reduced, placement is more difficult, and those benefits that trainees do realize are more likely to come at the expense of other workers who are displaced.

For some programs the estimated improvement in the economic situation of the trainees is large enough to recoup the cost incurred in training and, therefore, to justify the program on economic grounds alone. However, in cases where a program cannot be justified on the basis of posttraining earnings increases, there is no agreement on the extent to which the training should be subsidized.

Despite substantial expenditure of public funds for research and evaluation, there is only limited reliable information about the impact of training. Some of the largest and most important programs have been subjected only to very crude, preliminary investigations.

This paper was prepared by Jon H. Goldstein under the general direction of Alair A. Townsend, technical director of the subcommittee. Robert I. Lerman of the subcommittee staff made valuable comments and suggested improvements in the research at every stage of its development. The views expressed in this paper are exclusively those of the author and do not necessarily represent the views of the Subcommittee on Fiscal Policy, the Joint Economic Committee, individual members thereof, or its staff.

MARTHA W. GRIFFITHS,
Chairman, Subcommittee on Fiscal Policy.

THE EFFECTIVENESS OF MANPOWER TRAINING PROGRAMS: A REVIEW OF RESEARCH ON THE IMPACT ON THE POOR

BY

JON H. GOLDSTEIN

(V)

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SUMMARY AND CONCLUSIONS

Manpower training programs are being used and expanded programs proposed as a technique for increasing the earnings of the poor. This paper reviews the experience of a number of training programs, serving a variety of clientele with a wide range of techniques, in order to offer a considered judgment as to the likely success of a massive training effort. Five programs are examined: Manpower Development and Training Act (MDTA), Neighborhood Youth Corps (NYC), Job Corps, Job Opportunities in the Business Sector (JOBS), and the Work Incentive Program (WIN). The methodological section reviews the criteria used for evaluation, the information required to isolate the impact of training, the difficulties of identifying and estimating costs and benefits, and the problems of applying the results of these studies to the low-income target population of an expanded training effort.

Methodology

1. Program Goals

One of the difficulties of evaluating training programs for the poor is the lack of agreement on specific program objectives. One goal is to improve the distribution of income in society. This could be accomplished through direct transfer payments, but it is considered preferable to equip the poor with the skills to provide for more of their own economic needs. However, some training programs are expensive, and the costs exceed the benefits. Therein lies the problem in evaluating training programs: there is no agreement on the extent to which they should be subsidized.

2. Economic Efficiency

A program is economically efficient if the benefits it generates exceed the costs. Since benefits and costs are realized at different times, they must be discounted at some appropriate interest rate to make them comparable.

3. Definition and Measurement of Benefits and Costs

The definition of benefits and costs differs depending upon whether the program is being evaluated from the point of view of society, the taxpayer, or the trainee. The emphasis throughout this paper is on social benefits and costs. The social cost of a training program is defined as the value of the output which could have been produced with the resources actually employed in training. The social benefit of training is defined as the change in full employment net national product plus any externalities (indirect benefits, such as intergenerational effects or reduced crime). Since it usually is not possible to estimate the value of externalities, authors reluctantly settle for increases in earnings from increased wages or employment as the measure of social benefit.

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4. Isolating the Impact of Training

Isolating the impact of manpower programs is very difficult, because the evaluation process occurs in the changing and unconfined setting of the entire economy, not in a controlled laboratory environment. The impact of training varies with the characteristics of the individual trainee and existing economic conditions. In order to isolate and measure those changes in the enrollees' economic situation attributable to training alone, a study must control for those demographic characteristics and external influences which affect labor force experience. Studying a large number of trainees is expensive, however, and the limited number of observed cases often prevents analysis of some interesting socio-demographic groups.

5. Control Group

A crucial element in the design of any study is the control group, because the results can be very sensitive to its composition. The control group is a reference point, and the difference between its situation and that of the trainee group in the posttraining period is used to measure the effect of training. Many of the studies examined failed to select an appropriate control group.

6. Length of Observation Period

The expense of a longitudinal study has severely limited the length of the observation period for most evaluations. Few studies track the participants for more than a year. It is common practice to assume that observed benefits will persist in future years. It is the rare program whose benefits are so apparent that such projections into the future are unnecessary. Several studies in West Virginia with observation periods of 2 to 4 years found that earnings and employment differences between trainees and the control group grew smaller with time. This finding serves as a warning against the ready acceptance of conclusions based on benefit projections far into the future.

Manpower Development and Training Act

MDTA is the oldest training program. Its enrollees are a heterogeneous group, and training has been both institutional and on-the job. But its very breadth makes it an unwieldy subject, and precludes any simple, unqualified determination of its effectiveness.

MDTA has been studied extensively, and seven of the better efforts are reviewed in this paper. Each study has some feature which makes it precarious to generalize the findings. Dated information, small sample sizes, local rather than national samples, and questionable control groups are some of the problems of the studies. Nonetheless, they constitute the best information available.

1. Economic Impact

With one exception all of the studies reviewed estimated positive and relatively large internal social rates of return for MDTA.[1] The estimates range from 6.3 to 138.0 percent. Even the exception (Sewell, 1971) estimated a large return for on-the-job training (49 percent), the small return being for institutional training (6.3 percent). These estimates are based on the assumption that the earnings increases from training last for 10 years. It is quite unlikely that such con-

[1] See p. 22 for the definition of internal social rate of return.

sistent results would have been obtained if MDTA were not achieving some success. It is worth noting, however, that if the benefits had been assumed to last only 5 years instead of 10, several of the studies would have predicted unacceptably low rates of return (rates of return too low for economic efficiency). Observation periods have been too short to determine how long training benefits last.

These estimated rates of return are for entire programs, and as such do not reveal the very different impact that training has on various socio-economic groups. The impact of training varies with the type of training, the characteristics of the trainees, and existing economic conditions.

2. Economic Impact on the Disadvantaged

Disadvantaged persons do experience earnings increases as a result of exposure to training.[2] Regarding the size of benefits to the disadvantaged relative to those who are not disadvantaged, the evidence is mixed. Most of the studies reviewed found that trainees who were disadvantaged experienced gains from training at least as large as those for persons who were less hampered in the labor market. One large study (Smith, 1970) found the reverse. Every study estimated an improvement in the economic position of the disadvantaged large enough to recoup the social cost incurred in training. At the very least, training for this group generated a small, positive rate of return. The smallest estimate of the internal social rate of return was 3.5 percent (assuming that earnings differentials persist for 10 years).

A detailed summary of the differential effect of training by socio-economic characteristic follows the program summaries.

3. Institutional vs. On-the-job Training

The evidence examined supports the widely held belief that on-the-job training is superior to institutional training, but this evidence is neither extensive nor conclusive.

There is only one reliable study of this issue (Sewell, 1971) which is based on a control group comparison. The sample is relatively small, drawn entirely from a rural setting, and almost exclusively Negro. This study found that on-the-job training led to a significant increase in the weekly earnings of both males and females ($7.40 and $14.50, respectively), while only the weekly earnings of male trainees were influenced by institutional training ($8.30). Although male earnings responded about equally to both types of training, the higher cost of institutional training in this particular program resulted in the conclusion that on-the-job training is a much better investment for men as well as women. Examining the differential impact of institutional training by sex, Stromsdorfer (1968) found that females had no significant earnings increase, while males had large increases in both earnings and employment. It appears that women who undertake on-the-job training are more committed to the labor market than women who undertake institutional training.

MDTA program statistics for the period 1963–71 tend to support these results: 86 percent of MDTA on-the-job training graduates were employed 6 months after completing their training as opposed to 74

[2] The official definition of a disadvantaged person is "a poor person who does not have suitable employment and who is either (1) a school dropout, (2) a member of a minority, (3) under 22 years of age, (4) 45 years of age or over, or (5) handicapped." Manpower Report of the President, March 1970, p. 60.

percent for the institutional trainees. However, in the absence of an experiment in which enrollees are randomly assigned to the two training methods, one cannot be sure that a differential impact exists.

4. Sensitivity to Cyclical Economic Conditions

The effectiveness of training is very likely to vary directly with the demand for labor in the local labor market. Training probably creates a larger difference between the earnings of trainees and their control group at low unemployment rates than at high rates. Most studies attempt to correct for the downward influence that weak labor markets have on the *level* of earnings of both trainees and nontrainees. However, there have been no studies which estimate the differential impact of training on earnings at various unemployment rates.

One study (Smith and Wertheimer, 1971) documented the sensitivity of the employment rate of MDTA graduates to local employment conditions, but the impact seems puzzlingly small. A difference of 1 percent in State employment rates produced a 2¼ percent difference in the employment rate of current MDTA graduates. Both WIN and JOBS are much more sensitive to economic conditions.

5. Urban-Rural Differentials

The question of whether training has a differential impact on enrollees in urban and rural locations has not been well investigated. *A priori*, the wider market, the more diversified industrial structure, and the higher turnover in an urban area suggests that trainees there might enjoy an advantage. We were unable to find any treatment of this question in the literature. It is conceivable that no differential impact exists. Even if rural trainees are confronted with more limited opportunities, their newly acquired skills may increase their mobility, and permit them to migrate more easily. The question warrants investigation.

Neighborhood Youth Corps—In-School and Summer

There is only one benefit-cost analysis of the NYC in-school and summer programs (Somers and Stromsdorfer, 1970). NYC has been modified since this study was undertaken, and the conclusions may no longer be valid.

1. Post-High School Economic Benefits

The authors fitted two quite different models to the data, but used only one of these to make their estimates of the program's impact on employment and earnings. They attributed large post-high school economic benefits to NYC participation. For the total sample, the estimate of the increase in pretax earnings due to NYC participation was $831 during a period of 18.56 months, or $45 per month. Even if the earnings gains did not persist beyond this 1½ year period, an internal social rate of return of 90 percent is implied.

We think that of the two models fitted to the data, the authors chose the wrong one to estimate the benefits of the NYC program. The model which we consider to be more appropriate implies that there were no post-high school economic benefits from NYC participation.

2. Benefits by Program Component

NYC participants can be enrolled in any of three program combinations: in-school only, summer only, or both in-school and summer. If any of these were responsible for post-high school earnings increases (and that issue is in doubt), they were the in-school only and the combined in-school and summer components. There was no evidence that the summer-only enrollees benefited relative to their control group.

3. Differential Impact by Demographic Characteristic

Because the authors estimated differential benefits by demographic characteristic with a model we consider inappropriate, no summary of these differential impacts is given below. They are discussed in the text, however.

4. Educational Impact

The primary legislative function of NYC is to encourage continued school attendance. Research findings on the educational impact of NYC are uniformly discouraging, suggesting that the program is badly conceived as a solution to the dropout problem. Several authors found evidence that it actually reduced the probability of high school graduation. One study (Robin, 1969) concluded that the program was not influential in reducing the dropout rate, or increasing enrollees' educational aspirations, studiousness, or scholastic achievement. Work experience distracted students who already had low grades, causing them to further reduce the minimal amount of time they devoted to their studies. The determinants of the dropout rate are complex, and it appears that NYC is too simplistic a mechanism to be effective in reducing the incidence of school dropouts.

Neighborhood Youth Corps—Out-of-School

No analysis based on a national sample exists for the out-of-school program. Borus et al. (1970) have done a benefit-cost analysis of the program in five cities in Indiana. The localized nature of the study makes generalization hazardous. Its results suggest that the program is helping male school dropouts adjust in the labor market, but that the benefits to females are small.

Each hour of program participation increased annual earnings by an estimated 33 cents. Since enrollees averaged 520 hours in the program, expected annual benefits were $173.

Benefits varied widely by sex and level of education, with high school dropouts showing higher benefit-cost ratios than graduates. There was no evidence that training had a differential impact by race; whites and nonwhites benefitted equally from the program.

If the economically inefficient nature of NYC out-of-school training for women in Indiana proves to be universal, structural changes will have to be made to meet the needs of females. Women with 10 years of education (the mean level for the sample) who spent 520 hours in the program had expected annual earnings increases of only $83. Their male counterparts were expected to benefit by $562. At every level of education the expected earnings increases for women were not sufficient to generate social benefit-cost ratios greater than 1.[3] At every

[3] Except under assumptions which we consider unrealistic or inappropriate.

level of education the ratios for men were greater than 1, the most likely values lying in the range 2.4 to 3.3.

Job Corps

There have been two benefit-cost analyses of the Job Corps based on a national sample: Cain, 1968 and Resource Management Corp. (RMC), 1968. Their conclusions are not encouraging, but both studies have so many technical problems that the results are unreliable. If their estimates prove accurate, the Job Corps is economically inefficient. However, the technical problems are so great that it would not be judicious to assess the Job Corps on the basis of these studies.

1. Economic Benefits

Cain estimated annual gains in earnings at $203. Job Corps training is expensive, however, and these gains would have to have persisted for 42 years to generate a modest 5-percent internal social rate of return.[4] Cain's estimates were based on observations 6 months after the trainees left the program. When RMC examined the same sample 1 year later, they found that the earnings gains had declined so greatly that even if they were to last forever, they would not generate benefit-cost ratios greater than 1. More importantly, the gains were no longer statistically significant.

2. Technical Problems

a. The control groups are suspect.

b. The observation periods are short (6 months and 1½ years, respectively).

c. The observations are on 1966 trainees, only the second year of Job Corps operation. They may not reflect its current effectiveness.

d. Gross differences in earnings between the corpsmen and the control group were used to measure the impact of training. The estimates were not adjusted for the possible influence of personal differences or variations in local labor market conditions.

Work Incentive Program

The caliber of research on WIN is extremely poor. There have been no longitudinal studies of the labor force experience of WIN participants. No analysis has been conducted which uses a control group, and consequently there is no way to isolate the effect of exposure to WIN.[4a]

1. Data Problems Hampering Evaluation

The data available on WIN trainees are largely limited to job placement and dropout rates; posttraining earnings and employment information is extremely scanty. These data control for neither personal nor environmental variables, give no insight into income increases or welfare receipt decreases relative to a control group, and provide no basis for comparing benefits to costs. They cannot be used to estimate the impact of WIN on the trainees' economic situation.

[4] In 1967 the estimated social cost of training a corpsman for 5 months, the average length of participation, was $3,508. It cost $5,662 for 9 months, the normal term for graduation.

[4a] A longitudinal study of WIN which uses a control group was published too late for critical examination in this paper: Ronald E. Fine, et. al., *Final Report, AFDC Employment and Referral Guidelines*, Institute for Interdisciplinary Studies, Minneapolis, June 1972. The authors concluded that WIN services did not increase the earnings or employment of the trainees.

2. Program Placement and Dropout Rates

Placement and dropout rates do provide a basis for tempering expectations about the potential self-sufficiency of the AFDC population under the present structure of work incentives and legal sanction against those who refuse to participate. Successful completers (persons who were employed 3 to 6 months after being placed) have been a constant 20 percent of terminees until recently; during the period April 1971 to April 1972 they increased to 30 percent. Dropouts from WIN without good cause continue to average 21 percent of terminees. The comparable successful completion rate for MDTA has been considerably higher: 51 percent between 1963 and 1971. The client populations are quite different, however, and the comparison indicates nothing about the relative rates of return.

3. Differential Rates by Demographic Characteristics

Placement and dropout rates vary significantly by demographic characteristics. The patterns are generally consistent with one's intuition: WIN participants with characteristics which are indicative of a lack of maturity or family responsibility (such as youthfulness, not being a household head, having few dependents) or which put them at a disadvantage in the labor market (such as being female, a school dropout, or having little labor force experience) had greater difficulty locating employment and generally higher dropout rates. (See below for a detailed summary.) This is not to say that WIN was less effective in improving the employment prospects or increasing the earnings of these groups. Again, there is no way to determine from these data whether WIN had any impact at all.

4. Sensitivity to Cyclical Economic Conditions

WIN's ability to place trainees is very sensitive to cyclical economic conditions. At current enrollment levels a 1 percentage point increase in the national unemployment rate increases the number awaiting job placement by an estimated 3,000 people. This is about equal to the number of trainees who successfully complete the program each month.

5. Work Disincentives

The high benefit reduction rates confronting many AFDC recipients may discourage work effort and hinder the success of the program. Earnings above $30 a month are taxed at a two-thirds rate (although generous deductions for work expenses are allowed.) The benefit reduction rate is higher if the family is a recipient of other income-tested assistance. Regardless of the amount of his earnings, if an AFDC father is employed more than 100 hours a month, his family is ineligible for assistance.

6. Ineffectiveness of Penalties for Refusing Work or Training

If a person is referred to WIN, but refuses to participate, the law requires that his family's welfare payment be reduced and that, instead of an assistance check, the welfare agency must make direct payments to merchants for the majority of the family's expenses. These sanctions are ineffective for three reasons: (1) the penalty does not apply to mothers who volunteer for training, and most of WIN's clientele are volunteer mothers. (2) Because of the administrative expense of making direct payments to merchants and reluctance to

impose hardship on a family, welfare agencies frequently do not impose the penalty. (3) Because a family is ineligible for AFDC once a father is employed more than 100 hours a month, family income is often reduced less if the father refuses to participate in WIN and accepts the penalty than if he accepts employment.

7. Prospects for Future Success

WIN has been a small program relative to the size and growth of the AFDC population. Given the work disincentives, the virtual absence of penalties for noncompliance, the reluctance of employers to hire AFDC recipients, and the high national unemployment rate, it is remarkable that WIN's placement rate is as high as it is. Since there is evidence that WIN authorities enrolled persons who would be easiest to place, the prospects for improved placement rates and subsequent reductions in the welfare rolls through expansion of a structurally unaltered program are not encouraging.

8. Recommended Changes

The recent inclusion in the tax code of a tax credit to employers for 20 percent of the wages paid to WIN participants during their first year of employment should make it easier to place trainees. But the limits on this credit reduce its potential for increasing employment among welfare recipients. Twenty percent of wages are allowed as a credit up to a maximum of $25,000 per employer. (This is equivalent to the credit for only 25 full-time workers at a $2.50 hourly wage rate.) Thereafter the credit is halved to 10 percent, severely reducing the attractiveness of WIN graduates. Also, a tax credit provides no incentive to tax-exempt institutions and government agencies; a direct wage subsidy would.

Although the tax credit is a welcome improvement in WIN, plans for rigid enforcement of the sanctions against dropouts without simultaneous increases in work incentives seem ill-considered. An enrollee can always sit through training, and then avoid employment by making himself sufficiently unattractive to a prospective employer. In the absence of financial inducements, resistance from those compelled to participate can be expected. Finally, reducing the large number of WIN participants who dropout for legitimate reasons is going to require improved labor market conditions, longer periods of training to provide greater skills, and solutions to participants' health, transportation, and family-care problems, all of which may prove expensive.

Job Opportunities in the Business Sector

No controlled studies of the impact of JOBS on the employment and earnings of enrollees have been conducted. Even the number of persons reported by the Labor Department as placed through the program is suspect. The data that have been collected are unverifiable and unanalyzable.

1. Unverifiable Data and Exaggerated Claims of Accomplishment

The objective of the JOBS program is to place disadvantaged persons who need on-the-job training and supportive services in private industry jobs. In June, 1970, after 2½ years of operation, NAB and the Department of Labor were reporting 494,000 trainees hired under the

program and a retention rate of 47 percent. The Government Accounting Office (GAO) found that these statistics were unreliable. Detailed quarterly reports on trainees required from employers were not being provided, and the number of persons reported as hired frequently exceeded the number actually hired. In a significant number of cases the reported information could not be verified, because employers had maintained no records on the trainees.

The number of disadvantaged reported as hired was further distorted because employers themselves frequently certified trainee applicants as disadvantaged rather than referring them to the employment service for this determination. Consequently, a significant number of persons who were not disadvantaged were hired under the program.

2. Numerous Instance of Nonfulfillment of Contracts and Subversion of Program Goals

Although many firms are sincere in their efforts and committed to aiding the disadvantaged, a number of problems with the program are manifest. The objective of the JOBS program is not just to place the disadvantaged in the kinds of jobs they might have gotten anyway, but to train them and place them in jobs requiring significant skills. Two studies (GAO, 1971 and Greenleigh Associates, 1970) found that many of the jobs filled under the program were positions traditionally held by low-skilled and unskilled persons. Greenleigh concluded that most of the jobs pledged by employers were concentrated in occupations which historically have had high turnover rates. A significant number of employers did not supply the supportive services which were stipulated in their contracts and for which they were reimbursed. The GAO felt that responsibility for these problems lay largely with the Labor Department for inadequately monitoring the program and for rushing through contract negotiations with only limited consideration of the manner in which training and supportive services were to be provided.

3. Estimated Economic Impact

The Labor Department has drawn a random sample of 12,000 from the social security earnings records of JOBS employees, comparing their earnings for 1966 (prior to the inception of JOBS) with those for 1968 (the program's first year of operation). The mean earnings of these workers increased from $1,499 to $2,592, a difference of $1,100 and a 73-percent change. The number reporting no earnings decreased by 90 percent, and those with earnings between $4,000 and $6,000 increased by 50 percent.

4. Criticism of Estimated Economic Impact

These are impressive gross figures, and it would be hard to believe that the program did not account for a sizable portion of the gains in employment and earnings. Nonetheless, this is only a before-after comparison. The study of Social Security records had no control group, and did not correct for the influence of other variables.

Although it is possible that the program improved the status of individual trainees, their progress may have come at the expense of others who were displaced. In either an expanding economy or one with a competitive structure and downwardly flexible wages and prices, newly trained people can be absorbed easily. Where markets are not

competitive and wages and prices are downwardly inflexible, displacement is a very real possibility. Furthermore, the numerous instances of trainees holding unskilled jobs suggests that there may have been very little net increase in the number of disadvantaged persons employed throughout the economy. The program may have served merely as a subsidy to firms who filled vacancies created by the attrition of some of their low-skilled employees with other low-skilled workers. From the available data it is impossible to determine what the net impact was.

5. Sensitivity to Cyclical Economic Conditions

During the first 2 years of JOBS' operation unemployment rates were low (3.5–3.6 percent), and firms had difficulty filling vacancies. The increased contacts through JOBS between employers with vacancies and the Employment Service, WIN, and CEP may have increased employment among the disadvantaged during this period. But when unemployment rates began to rise in 1970, firms laid off workers and canceled JOBS contracts. Persons who had been placed through the JOBS program had little seniority and were among the first victims of the recession.

6. Recommendations for Improving JOBS

It is possible to design a decentralized, on-the-job training program which fulfills the objectives of JOBS. However, it requires intensive monitoring to insure that initially unskilled persons are trained for skilled positions. Applicants must be screened to assure that they are disadvantaged, training must be supervised, and payment to firms must depend at least in part on retention of the trainee in an acceptable job.

Unless workers are retained in skilled positions following the completion of training, it is very difficult to verify that they were trained. As it stands, the JOBS program provides no incentive for retaining the worker. Firms are paid a subsidy only during the training period. This creates an opportunity for employers with high turnover rates among their low-skilled workers to subvert the program. The solution to this problem is to make partial payment of the training subsidy conditional upon the employee being retained in an acceptable job for a specified period of time following training.

The recommended changes are likely to increase the cost of the program, as well as the benefits. Moreover, these changes do not guarantee that the improved status of the trainees will not come at the expense of other workers who will be displaced.

7. Difficult for Small Firms To Participate

A recent survey of 940 companies participating in JOBS revealed that only 2 percent were small firms employing fewer than 100 persons.[5] Small firms are reluctant to participate, because it is much more disruptive for them than for large corporations to hire and integrate into their labor force workers who require special training, supervision, counseling, and supportive services. Since almost half of all private sector jobs are in firms with fewer than 100 employees, the virtual exclusion of such firms from the program severely restricts JOBS' potential effectiveness.

[5] The Conference Board, *Employing the Disadvantaged: A Company Perspective*, New York, 1972.

As long as the primary objective of JOBS remains the training of disadvantaged persons for skilled positions, it is going to be difficult to involve small employers. They could be offered liberal incentive payments, but that might prove quite expensive. There are no similar obstacles which would prevent small firms from participating in a wage subsidy scheme, but a wage subsidy carries no assurance of training and placement in a skilled position.

Differential Impacts of Training by Demographic Characteristics

The evidence presented below for MDTA and NYC out-of-school is based on control group comparisons. No such study of WIN has been made, and, hence, no estimate of its impact is available. Differential placement and dropout rates for WIN participants are given, but these should not be interpreted to imply differential impacts from training.

1. Sex

MDTA.—The two studies conducted thus far found that males who were exposed to institutional training had significant increases in earnings, while females did not benefit. However, women had larger increases in earnings from on-the-job training than males.

NYC Out-of-School.—Females had very small earnings increases as a result of training ($83 annually for those with 10 years of education, the mean educational level). The estimated benefits for women were not sufficient to generate social benefit-cost ratios greater than 1. Males with 10 years of schooling expected annual earnings increases of $562. The benefit-cost ratios for men at every educational level exceeded 1.

WIN.—Women displayed lower placement rates, lower dropout rates, and higher rates of termination for legitimate reasons. The significantly lower dropout rate for women may reflect the fact that almost all female participants were volunteers. AFDC fathers were referred to WIN whether they wanted training or not, and hence, they may have been less motivated. Although women may have been more motivated, they face more barriers to employment. Greater family care responsibilities, the frequent breakdown of child care arrangements, and fewer employment opportunities may account for the higher rate of legitimate termination and the lower rate of successful placement.

2. Education

MDTA.—Training had a greater impact on the earnings of those with less education. Several studies found that training benefited high school dropouts more than graduates. At least two studies found that training had the greatest impact on those with only grade school education.

NYC Out-of-School.—High school dropouts showed higher benefit-cost ratios than graduates.

WIN.—High school graduates were significantly easier to place than high school dropouts. High school dropouts had higher dropout rates from WIN than any other educational group.

Those participants with the lowest levels of grade school education (1 to 4 years) had both the lowest placement rates and the lowest dropout rates. The low dropout rate for this group is particularly poignant, for it suggests they were not failing for lack of trying.

3. Race

MDTA.—The differential effect of training by race has not been well investigated. What little evidence there is (one study in Michigan with a sample size of 150) suggests that whites benefit more from training than blacks, but both groups experience increases in earnings. The explanation for the differential effect of training on the productivity of whites and blacks (if in fact one exists) is unknown. The most reliable investigations of the issue found that discrimination rather than motivation or inherent personal differences accounts for the inequality in earnings and employment.

NYC Out-of-School.—There was no evidence that training had a differential impact by race; whites and nonwhites benefited equally from the program.

WIN.—Placement rates for blacks and whites were identical at 21 percent, but other ethnic groups (American Indians, Mexican Americans, Puerto Ricans, and Orientals) had only a 15 percent rate. There were no significant differences in the dropout rates of women by ethnic origin, but nonwhite males had a dropout rate of 28 percent as compared to 19 percent for white males.

4. Age

MDTA.—There was no consistent relationship between age and the impact of training.

WIN.—Youths and older workers had more difficulty getting placed. Placement rates increased with age up to age 54, and declined thereafter. Dropout rates were very high for trainees less than 18 years old (33 percent for males, 27 percent for females), and declined with age thereafter. The dropout rate for males 65 and over increased sharply.

5. Indices of Maturity and Family Responsibilities

MDTA.—The two studies which investigated the issue found no consistent relationship between the effectiveness of training and marital status, status as a household head, or number of dependents.

WIN.—Placement rates were lower and the dropout rates higher for those participants who were not household heads, had never been married, or had few dependents.

6. Previous Labor Market Experience

MDTA.—Persons with a history of extensive unemployment prior to enrolling in MDTA had larger increases in earnings due to training than those who had been employed or those who had been unemployed for shorter periods. One study (Olympus Research Corp., 1971) found that those trainees with the lowest earnings and wage rates prior to training experienced the largest increases in earnings and wage rates.[6]

[6] This study did not use a control group.

WIN.—Placement rates increased and dropout rates decreased with years of previous work experience.

Thus, the pattern of WIN placement and dropout rates is generally consistent with one's intuition: participants with characteristics which indicate a lack of family responsibilities or of maturity or which put them at a disadvantage in the labor market had greater difficulty locating employment and generally higher dropout rates. But the pattern of differential training impacts for MDTA and NYC out-of-school participants is quite surprising. Persons with less education and those with recent lengthy spells of unemployment benefited more than those without these liabilities. Training has been successful among all age brackets and for persons with varying degrees of family responsibility; no consistent differential impact emerged. The results of the limited research on effects by race were mixed, with persons of all races showing improvement. Women did not seem to benefit from institutional training, but some studies found that they benefited more than men from on-the-job training.

Assessing the Results

Disadvantaged and low-income persons have responded to training and have become more self-sustaining. It is important that we retain our perspective, however. The results which we have been examining pertain to training programs during the last decade. Although the absolute number of trainees during that period was quite large, it is likely to be miniscule in comparison with the number for whom training would be specified if manpower programs were adopted as a mainstay of income maintenance policy. No one knows whether a massive training effort for the low-income and welfare population will generate a similar outcome.

Increased enrollments will make it difficult to duplicate the quality of past training programs. Instructors are a scarce resource, and attempts to hire more of them may increase the per capita cost of training. Selecting the positions for which participants should be trained is already an uncertain task, and the risk is multiplied as the program grows. Judgment errors will occur, creating excess supplies of some occupational skills.

Thus far, trainees have comprised a negligible proportion of the labor force, and the additional competitive pressure which they have exerted on wage rates has probably been small. A much more ambitious program of training for the low-income population would encounter increasing difficulties in getting graduates absorbed into the private sector. The most sanguine economic model (one which assumes flexible wage rates) predicts that employment is available for the trainees but at somewhat lower wage rates. This, of course, would reduce the rate of return from training. In a world encumbered with institutional restrictions, noncompetitive firms, and powerful unions, placement in the private sector becomes more doubtful and a supporting program of public employment may prove necessary.

Almost all trainees in the past have been volunteers. If compulsory training were instituted for particular categories of welfare recipients, changes in the motivation, if not the qualifications, of the "participant" population could be anticipated. Even if future trainees were as capable as those in the past, resistance to mandatory training could

produce results altogether different from those which have been witnessed previously. In addition, if women responsible for young children are included in the mandated population, the problem of insufficient child care arrangements will have to be confronted, for these women to be able to remain active in the labor force.

Fear of inflationary pressures has resulted in reluctance by the administration to pursue a policy of full employment demand. In the long run, training programs should reduce some of the skill shortages which help fan inflation, but it would be naive to expect them to eliminate the problem. Given the sensitivity of the success of manpower programs to the level of economic activity, continuation of the current macroeconomic policies will make it impossible to realize the estimated benefits of training. If unemployment is not reduced below the 5 percent level, much of the $1.6 billion planned for manpower programs in fiscal 1973 could be better spent on job creation.

We have couched our discussion in terms of increases in earnings due to training and the rates of return on investment. These are important measures of program success and economic efficiency, but their significance can be overemphasized. Although some of the research results suggest that the gains in earnings have been large relative to costs, they have not been large by conventional, social standards. It is sobering to note that even those studies with the most optimistic results estimate average posttraining annual earnings levels well below the poverty line. In a study of MDTA trainees in North Carolina (Sewell, 1971), the average posttraining annual earnings for the trainees was $2,406, a gain of $433 over the nontrainees, but still $471 below the poverty line for this group. In terms of the absolute and percentage gain in annual earnings as well as the rate of return on investment, female on-the-job trainees were the most successful of the North Carolina participants. Yet their posttraining annual earnings averaged only $1,857. In a more recent sample (Olympus Research Corp., 1971), MDTA trainees did somewhat better, averaging $3,100 in posttraining annual earnings. But this was still over $800 below the relevant poverty line. If child care and work expenses have to be financed from these earnings, there is not much left for the amenities of life. Training does reduce the poverty gap, but continued income supplementation is likely to be necessary for the graduates.

Improving the Evaluation Process

The robust expenditures for research and evaluation of training programs ($179.4 million from fiscal 1962 through 1972)[7] are a disturbing contrast to the anemic set of conclusive and reliable findings. Although some of the data may be necessary management information, much of what is collected as a matter of course by program administrators cannot be used to estimate the impact of training and determine the effectiveness of the program.

Among the most glaring deficiencies are inappropriate control groups and short observation periods. An appropriate control group is essential if the impact of training is to be isolated and distinguished from the influence of other factors. Frequently, studies are undertaken without any control group. When a control group is included in the

[7] Federal funds spent for the evaluation, research, and development of training programs by the Department of Labor, the Department of Health, Education, and Welfare, the GAO, and the Office of Economic Opportunity.

design, it is almost never established at the time that enrollees enter the training program. Evaluators are called in after training has occurred, and are confronted with the almost impossible task of constructing a control group with the pretraining characteristics and experience of the trainees.

Observation periods rarely last more than a year and usually less. This is too short to determine how long benefits last or whether they are stable, increase, or decrease. Evaluators are forced to base their estimates of program effectiveness on uneasy assumptions about the duration of earnings gains. Tracking trainees and a control group over an extended period of time is an expensive proposition, but it is probably no more expensive and certainly more useful than much of the data collection and evaluation which has been conducted in the past.

Because the structure of programs and the characteristics of their clientele change over time, a single evaluation, even one with an optimal design, conducted at one point in time does not provide reliable information about program effectiveness. A standardized, ongoing evaluation procedure should be established.

Finally, enabling legislation usually assigns the task of evaluation to the program administrators. Separation of powers is a well-accepted and venerable principle, and its application is as appropriate here as elsewhere. Administrators are understandably anxious to depict their programs as successful, and evaluations conducted by them (no matter how conscientious they may be) cannot escape being suspected of bias. An independent agency, accountable to Congress, should be responsible for evaluation.

INTRODUCTION

Manpower training programs are being used, and expanded programs are being proposed as a technique for increasing the earnings of the poor. Both the administration's proposal for welfare reform (the Family Assistance Plan) and the Senate Finance Committee's counter-proposal (the Guaranteed Job Opportunity for Families) include provisions for training. The Social Security Act has been recently amended to require all welfare recipients (with certain specified exceptions) to register with the Labor Department for work or training.

We now have had a decade of experience with manpower programs, and a voluminous evaluation literature has emerged from the attempt to determine their impact. Although in the past trainees were drawn only in part from the poverty population, it seems appropriate to examine the accumulated evidence from these programs before unrealistic expectations are generated regarding their powers to metamorphosize the poor. Thus, a review has been made of the experience of a number of training programs, serving a variety of clientele with a wide range of techniques, in order to offer a considered judgment as to the likely success of a massive training effort.

This study is not a comprehensive review of the evaluation literature.[1] Because the Subcommittee staff's research is focused on solutions to the poverty problem, some of the better and more recent studies have been examined for particular insights into the impact of training on low-income and disadvantaged persons.[2]

Manpower programs may have been oversold in the past, fostering the illusion that they would (1) eliminate unemployment which was unresponsive to economic growth, (2) mitigate the severity of the inflation-unemployment trade-off by increasing the productivity and the occupational and geographic mobility of the low-income population, and (3) reduce the duration of unemployment experienced by those displaced by automation. To these objectives recently has been added responsibility for stemming the growing tide of welfare recipients by making the poor self-sufficient, and in the process so improving the character of the trainees that the incidence of a variety of antisocial activities (urban crime, parental desertion, dropping out of school, drug addiction, and urban blight in general) will be perceptibly diminished.

To note that these problems are still with us is to say nothing significant about the degree of success of the training programs. The evaluation of these programs is an extremely complex task, the evidence is not all in, and the blizzard of statistical information has to be considered with circumspection.

[1] For a comprehensive bibliography see Ernst W. Stromsdorfer, *Review and Synthesis of Cost-Effectiveness Studies of Vocational and Technical Education*, The Center for Vocational and Technical Education, Ohio State University, Columbus, Ohio, January 6, 1972, pp. 177–215.

[2] The official definition of a disadvantaged person is "a poor person who does not have suitable employment and who is either (1) a school dropout, (2) a member of a minority, (3) under 22 years of age, (4) 45 years of age or over, or (5) handicapped." Manpower Report of the President, March 1970, p. 60. In addition to these characteristics, we will be concerned with any trait which is likely to reduce one's marketability or meet with discrimination in the labor market, e.g., being female or having an arrest record.

Literature on five programs was examined: Manpower Development and Training Act (MDTA), Neighborhood Youth Corps (NYC), Job Corps, Job Opportunities in the Business Sector (JOBS), and the Work Incentive Program (WIN).[3] MDTA and NYC are the largest of the manpower training programs, and have been in operation the longest. MDTA offers a wide range of institutional and on-the-job instruction; it serves disadvantaged persons, although not exclusively. WIN treats only welfare recipients. NYC, JOBS, and Job Corps deal exclusively with disadvantaged persons, though each has its own structure and training methodology. JOBS is a private venture which is federally funded. The majority of Job Corps centers train youths at sites away from their home environment. NYC provides work experience, earnings, and training to high school students and dropouts, and encourages them to continue their education.

Information on the number of training program participants between 1963 and 1971, the Federal funds obligated to train them, the MDTA completion and posttraining employment record, and some selected characteristics of trainees appears in table 1. It is clear that (1) substantial public resources ($6.8 billion) have been devoted to exposing a sizable number of people (6.1 million) to training services during the period, (2) the percentage of MDTA graduates who were employed 6 months after completing their training has been on average somewhat higher for on-the-job than for institutional training (86 and 74 percent respectively, the percentages remaining fairly constant over time), and (3) since an increasing proportion of the clientele have displayed characteristics which are likely to place them at a disadvantage in the labor market, the target population is being reached.

[3] See Supplementary Material for a description of the operation of these programs and the characteristics of the trainees served by them.

TABLE 1.—TRAINING PROGRAM OPERATING STATISTICS, TRAINEE CHARACTERISTICS, COMPLETIONS, AND POSTTRAINING EMPLOYMENT, FISCAL YEARS 1963–70

[Thousands]

1st-time enrollments	Total	1971	1970	1969	1968	1967	1966	1965	1964	1963
Total (thousands)	6,143.9	1,412.5	1,051.4	1,000.7	780.8	833.3	658.7	294.8	77.6	34.1
Manpower Development and Training Act	1,706.2	254.8	221.0	220.0	241.0	265.0	235.8	156.9	77.6	34.1
Institutional training	1,134.0	155.6	130.0	135.0	140.0	150.0	177.5	145.3	68.6	32.0
On-the-job training	572.2	99.2	91.0	85.0	101.0	115.0	58.3	11.6	9.0	2.1
Neighborhood Youth Corps	3,310.8	740.2	482.1	504.1	467.3	556.3	422.9	137.9		
In school	779.3	120.0	74.4	84.3	118.3	166.8	160.8	54.7		
Out of school	631.6	53.0	46.2	74.5	93.8	161.6	166.9	35.6		
Summer	1,899.9	567.2	361.5	345.3	255.2	227.9	95.2	47.6		
Operation Mainstream	69.3	21.9	12.5	11.3	12.6	11.0				
Public Service Careers	60.0	47.3	3.6	3.8	4.3	1.0				
Special impact	5.3			2.7	2.6					
CEP	383.8	93.7	110.1	127.0	53.0					
JOBS (Federally financed)	230.6	92.6	86.8	51.2						
Work Incentive Program	285.5	112.2	92.7	80.6						
Job Corps	92.4	49.8	42.6							
FEDERAL OBLIGATIONS										
Total (thousands)	$6,772,706	$1,485,466	$1,360,183	$1,029,730	$802,173	$795,950	$628,407	$414,247	$142,111	$56,070
Manpower Development and Training Act	2,363,948	335,752	315,931	272,616	296,418	298,247	339,649	286,585	142,111	56,070
Institutional training	1,904,414	263,936	246,083	207,795	218,251	215,492	281,710	249,348	135,525	55,219
On-the-job training[1]	428,708	60,285	59,860	59,111	74,571	82,659	57,939	37,157	6,586	851
Part-time and other training	30,826	11,531	9,988	5,710	3,596	96				
Neighborhood Youth Corps	2,125,514	426,453	308,050	320,696	281,864	348,833	265,237	127,742		
In school	NA	58,052	59,242	49,048	58,908	67,448	NA	NA		
Out of school	NA	115,195	97,923	122,246	95,889	147,826	NA	NA		
Summer	NA	253,206	150,885	147,927	126,677	133,306	NA	NA		
Work training in industry	NA			1,475	390	253	NA	NA		
Operation Mainstream	209,540	71,550	51,043	41,000	22,319	23,628				
Public Service Careers	222,592	91,636	87,067	18,460	7,557	15,573				
Special impact	10,138			1,100	2,038	7,000				
Concentrated Employment Program	665,453	166,752	187,301	114,220	93,057	78,441	25,421			
JOBS (federally financed)	592,870	169,051	162,152	160,821	89,920	24,298				
Work Incentive Program	252,682	64,085	78,780	100,817	9,000					
Job Corps	329,969	160,187	169,859							

SELECTED CHARACTERISTICS										
MDTA: Number (thousands)	1,678.7	227.3	221.0	220.0	241.0	265.0	235.8	156.9	77.6	34.1
Percent:										
Female	38.4	36.5	37.9	40.7	39.2	38.8	38.3	38.3	39.0	35.2
Age 21 or less	37.7	38.3	36.3	37.0	37.4	37.7	38.7	42.3	34.4	25.6
Age 45 or more	10.2	9.0	9.7	10.2	10.7	10.6	10.6	10.1	10.6	10.4
High school dropout	35.4	35.2	37.4	37.3	37.9	35.3	34.0	33.8	32.8	29.9
8th grade education or less	16.2	13.3	15.7	17.9	17.6	16.4	15.8	18.0	14.1	11.0
Poor	56.3	59.8	55.8	----	----	----	----	----	----	----
Public assistance recipient	9.7	12.9	9.9	10.3	9.5	8.1	9.1	9.9	8.9	7.7
Nonwhite	36.9	40.3	37.7	42.1	43.6	34.8	34.1	31.6	29.4	23.1
NYC,[2] JOBS, Job Corps, WIN: Number (thousands)	----	307.6	303.6	275.4	137.6	172.9	187.2	----	----	----
Percent:										
Female	----	45.4	48.4	46.8	50.9	48.4	43.0	----	----	----
Age 21 or less	----	56.0	60.5	64.3	98.5	100.0	100.0	----	----	----
Age 45 or more	----	3.4	3.2	3.7	0	0	0	----	----	----
Years of school completed: 8 or less	----	22.9	25.5	24.4	27.6	27.2	24.9	----	----	----
Years of school completed: 9 to 11	----	50.2	53.1	55.9	65.6	63.4	55.9	----	----	----
Poor	----	100.0	100.0	100.0	100.0	100.0	100.0	----	----	----
Public assistance recipient	----	53.0	48.3	47.6	28.1	26.4	27.5	----	----	----
Nonwhite	----	55.4	60.6	59.5	49.8	53.0	51.8	----	----	----
MDTA										
Total:										
Enrollments	1,706.2	254.8	221.0	220.0	241.0	265.0	235.8	156.9	77.6	34.1
Completions	1,123.1	135.9	147.0	160.0	164.2	192.6	155.7	96.3	51.3	20.1
Employed 6 months after completion	878.5	105.1	115.3	124.0	127.5	153.7	124.0	73.4	39.4	16.1
Institutional training:										
Enrollments	1,134.0	155.6	130.0	135.0	140.0	150.0	177.5	145.3	68.6	32.0
Completions	742.0	90.3	85.0	95.0	91.0	109.0	117.7	88.8	46.0	19.2
Employed 6 months after completion	550.2	65.9	62.0	71.0	64.5	80.0	89.8	66.9	31.8	15.3
On-the-job training:										
Enrollments	572.2	99.2	91.0	85.0	101.0	115.0	58.3	11.6	9.0	2.1
Completions	381.1	45.6	62.0	65.0	73.2	83.6	38.0	7.5	5.3	.9
Employed 6 months after completion	328.3	39.2	53.3	53.0	63.0	73.7	34.2	6.5	4.6	.8

[1] Includes the JOBS—Optional Program (JOP) which began in fiscal 1971 and the MDTA on-the-job training (OJT) program which ended in fiscal 1970 except for national contracts.
[2] NYC out-of-school.

Note.—NYC data are for out-of-school only. NYC is reported here on a September to August basis; all other program data are for the fiscal year. This nonuniformity was ignored in the aggregation, and the period September to August was defined as the fiscal year for the NYC data.

Source: Manpower Report of the President, April 1971.

These figures are fairly unrevealing as a source for assessing the effectiveness of the training programs. We would like to know what the rate of return on the Government's investment was, whether the trainees experienced an increase in income, how much of any increase was attributable to the instruction which they received, and whether their posttraining work experience was lengthy and stable or merely a flirtation with employment.

The beginnings of answers to questions like these can be found in the technical studies which we have examined. In order to provide a common basis for discussion we begin with a methodological section, reviewing the criteria used for evaluation, the information required to isolate the impact of training, the difficulties involved in identifying and estimating costs and benefits, and the problems associated with making inferences from the results of these studies to the low-income population which is the likely target of an expanded training effort.

A Framework for Evaluating Training Programs for the Poor

Program Goals

One of the difficulties associated with evaluating training programs for the poor is the lack of uniform agreement (among policymakers and researchers alike) on the specific objectives of the programs. One of the goals is ethical: to improve the distribution of income in society. Most of those eligible for training are considered to be at a relative disadvantage in the labor market, and the training programs attempt to correct this inequity by improving the trainees' earning capacity.

However, there are alternative means of correcting a maldistribution of income; namely direct transfer payments. This society has a deep-seated commitment to the work ethic, and other things being equal, it is considered preferable to equip the poor with the skills to provide for more of their own economic needs. Other things are not equal, however. Resources are scarce, government budgets are tight, and there are limits to what we are willing to spend implementing the principle of self-support. The conflict is clear: we would like to reduce the incidence of poverty, and we would like to do it by involving the poor in the mainstream of economic activity. But we are also concerned about efficiency, and the cost of training is a very relevant consideration.

The conflict may be clear, but the limits of our preference for self-support are not. Some training programs are expensive relative to the benefits, but it is uncertain how much more society is willing to pay to achieve a given income increase for the poor through training rather than through some alternative means. Therein lies the problem in evaluating training programs as an antipoverty device.

Although there is no agreement on the extent to which training should be subsidized, there are some generally agreed upon approaches to evaluating programs on the basis of their economic efficiency. A program is economically efficient if the benefits it generates exceed the costs. Training programs are an investment in the formation of human capital. Society has released some of its scarce resources (plant and equipment, instructors, workers to be upgraded) from their task of producing current output, and has devoted them to training workers. The allocation of these resources is efficient if in the future

the human capital can produce output whose value (discounted [4] at some appropriate interest rate) is sufficient to cover the cost of the investment (the lost current goods and services).[5] This is the general principle applied throughout our examination of training programs.

It is essential to remember, however, that even if the benefits of a program are so small relative to the costs that the criterion of economic efficiency is not satisfied, the program cannot be dismissed as a possible approach to the poverty problem for three reasons: (1) Direct transfer payments are not administered without cost, and the benefits and the costs of such programs would have to be examined in order to make a comparison; (2) society's predisposition to increase the incomes of the poor through their own work efforts might be sufficient to warrant the extra costs involved in training; and (3) it is likely that there are important, indirect benefits from training (e.g., intergenerational effects and reduced crime) which cannot be measured.[6]

The Evaluation Process

Any benefit-cost analysis has four phases: specifying program objectives, defining appropriate concepts of benefits and costs, choosing criteria to evaluate the investment program on the basis of these concepts, and measuring the benefits and costs. The theoretical criteria for evaluating investments are well developed, and will be discussed briefly. Although enumerating the benefits and costs is basically an accounting procedure, some understanding of economics and the workings of an economy is required to avoid improper inclusions or omissions. After all, we are attempting to isolate and assess the impact of manpower programs not in a sterile, controlled laboratory environment, but in the changing and unconfined setting of the entire economy. This greatly increases the number of variables and their interactions. The relevant outputs and expenditures are altered by the perspective from which one examines training programs, that of society as a whole, that of the taxpayers who finance the project, or that of the recipients of training. This topic will be treated in some detail. Of the three phases, the estimation process is subject to the most uncertainty, and the dimensions of the problem are increased by the difficulty of approximating a controlled, experimental setting.

Investment Criteria

There are three well-established criteria which are used to evaluate investment projects: the benefit-cost ratio, the net present value, and the internal rate of return. The benefit-cost ratio results from dividing the discounted future benefits by the discounted costs.[7] A project

[4] Discounting is the opposite of compounding. It is a procedure for determining the present worth of output which will not be available until some future time. Society prefers $1 worth of goods and services now to those same commodities a year from now. If we devote $1 worth of resources now to manpower training, we will have to get output valued at more than $1 in the future, to consider this a worthwhile investment. The interest rate is normally used as a measure of the minimum amount that an efficient investment will have to return in the future. For example, a person with $1 in a saving account that pays 5 percent will have to be offered more than $1.05 next year if he is to be induced to invest his funds. (This is compounding.) By the same token, if the interest rate is 5 percent, $1.05 a year from now has a present or discounted value of $1.

[5] A benefit-cost ratio greater than one implies that the resources have been used in an economically efficient, but not necessarily optimal, manner. We have taken resources which were being used to produce output worth, say, $1, and have allocated them to a preferable use, as evidenced by the fact that we obtained output valued at more than $1. This is not necessarily the optimal allocation of these resources, however, for there may have existed a use which would have produced even more highly valued future output.

[6] For an articulate discussion of the problem of conflicting social goals and the evaluation of training programs see David O. Sewell, *Training the Poor, a Benefit-Cost Analysis of Manpower Programs in the U.S. Antipoverty Program,* Industrial Relations Center, Queen's University, Kingston, Ontario, 1971, pp. 51–52.

[7] See footnote 4 for an explanation of discounting.

is economically efficient if this ratio exceeds one. The net present value is simply the discounted or present value of the benefits less the discounted costs; if this difference is positive, resources are efficiently allocated. Finally, the internal rate of return is that interest rate which equalizes the present value of the benefits and costs. If the project yields an internal rate of return higher than the interest rate which the investor could have received for lending his funds, the rate of return is acceptable. These criteria serve essentially the same purpose, and all three are used by the authors of the studies under discussion.[8] In cases where budget limitations preclude undertaking all efficient projects, they should be ranked and chosen from according to their expected net present values. (An example illustrating the use of these investment criteria appears in Supplementary Materials, Section II.)

Since the interest rate enters explicitly in the determination of present values and is implicitly the standard of comparison for the internal rate of return, use of these criteria requires specifying a value for the rate of interest. As with benefits and costs, the conceptually proper interest rate depends upon the viewpoint from which the project is being evaluated (that of the taxpayer, society, or the trainee). However, regardless of the perspective, there is no general agreement on the correct value for the interest rate, and hence some latitude is allowed the evaluator. Authors normally choose a range of values (usually between 5 and 15 percent). The Office of Management and Budget uses a 10-persent rate on all government projects.

It is not possible to use benefit-cost analysis to determine the degree of success of a project when some of the goals established for the project are noneconomic, i.e., results that cannot be assigned monetary values. One such goal has already been discussed: income redistribution. Another example occurs in the case of the NYC: increasing the probability that a trainee will complete his or her education. If a project is economically efficient and there is evidence that the noneconomic goals are also being fulfilled, there is a strong presumption that the project should be continued. A problem arises only when some of the objectives are not being met and policymakers have not supplied a set of relative weights for the multiple goals.[9] In such cases the various outcomes can be discussed, but the project cannot be fully evaluated.

Definition of Benefits and Costs

A definition of benefits and costs should account for all the resources used by a project and all of the changes which occur as a result of it. There is considerable variation in the list of items identified as benefits and costs by authors of manpower evaluations. Such diverse things as increases in the earnings of the trainees, secondary increases in employment due to multiplier effects, and increased tax revenues accruing to the Goverament are counted as benefits, and the definition of costs ranges from the forgone earnings of enrollees in training programs to the Government funds expended to finance programs. The

[8] Considerable controversy exists over which of these criteria is the correct one. Depending upon the circumstances associated with a particular investment project, there are grounds for distinguishing between them. For a thorough summary of the problem and the conditions under which each is preferable see Teh-wei Hu, Maw Lin Lee, and Ernst W. Stromsdorfer, *A Cost-Effectiveness Study of Vocational Education*, Institute for Research on Human Resources, the Pennsylvania State University, University Park, Pa., March 1968, pp. 49–59.

[9] Assigning relative weights is essentially the same thing as giving monetary values to the noneconomic outcomes.

disparity is such that the same item may be counted as a benefit in one evaluation and a cost in another.[10]

For the most part these are legitimate differences, explainable by the dissimilar objectives of the evaluators. If the impact of the program on society as a whole is being estimated, a very different set of relevant benefits and costs emerges than if only the cost to and effects on the trainees or the taxpayers are considered. For instance, increases in posttraining tax collections and in the Government's share of training expenses are relevant for evaluation from the point of view of the taxpayer. From the point of view of the trainee the benefit is the increase in his disposable income and the principal cost is his foregone earnings during the program. Definitions and measures of the relevant benefits and costs from each of the three perspectives are discussed below.

A somewhat more subtle problem, which has not been well recognized and has caused considerable confusion, is whether benefits should be defined as the impact of training on the actual *output* of goods and services or on the *capacity* to produce output, i.e., the potential increase in output at full employment.[11] This philosophic difference is not minor, and has serious implications regarding what should be counted as a benefit or a cost of training.

If the increase in output or in net national product (NNP) is the definition adopted, then such things as multiplier effects must be counted as benefits of the training program. (Multiplier effects are increases in employment and output which occur as a result of the initial government expenditures on training programs, as well as increases which occur because the successful trainees have higher earnings, and will want to consume more, and additional persons will have to be employed to produce these newly demanded goods.) If a graduate of a training program appears so attractive to industry that he is hired to replace an existing employee, then the displaced worker's lost income will have to be subtracted from the benefits. Similarly, if a trainee vacates a job when he enters the training program, and the vacancy is then filled by some formerly unemployed worker (the so-called vacuum effect), then the foregone earnings of the trainee cannot be counted as a cost from society's point of view, because there has been no opportunity cost (reduction in production) as a result of training him. In general then, if one wants to measure the *actual* output increases and the *actual* costs associated with the initiation of a training program, one must count the entire expansion in output as a benefit and the entire contraction as the cost.[12]

This may be a proper question of concern to government officials, and one that shall be discussed, but it hardly seems like an appropriate method of assessing the efficacy of training programs. There is nothing remarkable about a multiplier effect. Any exogenous expenditure can generate a real increase in output, given the existence of excess capacity and unemployment in society. Long term unemployment experienced by workers displaced by trainees is not the fault of a training program, but rather of inappropriate macroeconomic stabilization policy. Similarly, it hardly seems reasonable to blame a training

[10] For example, stipends to trainees are a cost from the Government's point of view, but they are a benefit from the trainees' viewpoint.

[11] Einar Hardin, "Benefit-Cost Analysis of Occupational Training Programs: A Comparison of Recent Studies," *Cost-Benefit Analysis of Manpower Policies*, Proceedings of a North American Conference, May 14–15, 1969, G. G. Somers and W. D. Wood (ed.), Industrial Relations Center, Queen's University, Kingston, Ontario, 1969, p. 101.

[12] Ibid.

program, when trainees who have had their marketable skills increased cannot find jobs because cyclical unemployment is so high. It is not the province of training programs to generate sufficient aggregate demand to maintain a full employment economy. That is the responsibility of monetary and fiscal policy, and training programs should neither be given credit for contributing to the creation of adequate aggregate demand, nor be condemned because it has not been achieved.[13] The optimum context for evaluating training programs would seem to be the impact on the capacity of society to produce additional output.[14]

Social benefits.—From the point of view of the entire economy the benefits from training are defined as the change in full employment net national product plus any externalities.[15] The principal externalities are possible reductions in crime, and intergenerational effects, e.g. the children of successful trainees will be more productive in the future or engage in less antisocial behavior because their family income has been increased, they grow up in a healthier environment, they receive more education and health services, etc. Only very crude attempts have been made to estimate values for these externalities, and the researchers generally conclude that the benefits are small enough to be "safely" ignored.[16] Given the preliminary nature of the research on calculating externalities, they necessarily are ignored by all evaluations of manpower programs. It would be premature to conclude that such omissions are inconsequential; no one knows what their magnitude is.

There are three statistics which are used to measure the social benefits from training: changes in earnings, wage rates, and employment. While no one of these alone is a comprehensive measure of benefits, all three provide useful insights about the effect of training. Both wage and earnings changes understate the worker's increased productivity, because neither includes fringe benefits or employer contributions to Social Security.[17] Since it is difficult to obtain data on fringe benefits, most authors settle for wage and earnings changes. However, ignoring employer Social Security taxes may not be inconsequential.

Some authorities contend that an increase in earnings only indicates a productivity increase, if it is largely accounted for by an increase in

[13] One of the functions of manpower programs is to attack structural unemployment problems, and thereby ease the severity of the inflation-unemployment trade-off. This simplifies the task of maintaining full employment, but it is quite distinct from the responsibility for inducing adequate demand.

[14] Structuring the evaluation process in this manner creates a problem which is difficult to resolve. If the evaluation is conducted in a non-full-employment setting, observed prices for resources and outputs may differ from their full employment, equilibrium values. This could create unidentifiable biases in the estimates. If one believes that wages and prices are downwardly inflexible, then the problem is mitigated for situations of unemployment. No ready solution presents itself for inflationary situations, however.

[15] An external economy occurs if person A is better off as the result of an action by person B, the two having not engaged in a transaction. A diseconomy occurs if person A is worse off.

[16] Ribich examines the influence that additional years of education for parents have on their children's education, and then infers an income relationship based on other studies. He concludes that the benefits are small. Thomas I. Ribich, *Education and Poverty*, the Brookings Institution, Washington, D.C., 1969, pp. 101–107. Belton Fleisher, "The Effect of Income on Delinquency," *American Economic Review*, March 1966, pp. 118–137, examines the effect of increases in income on delinquency rates in low-income areas. Estimating cost savings on the basis of Fleisher's results, Ribich finds that a community saves only $1,300 in police expenditures if income increases by $300,000.

[17] Hardin, op. cit., p. 101.

wages, rather than an increase in the duration of employment.[18] It is argued that a wage increase is a more significant gage because it indicates that the employer thought that the worker had become more valuable. On the other hand, increases in earnings due solely to increases in employment at the same wage rate are often only a reflection of aggressive efforts on the part of placement officers or screening of job applicants by employers on the basis of a false criterion (Employers often use a credential such as a school diploma or a certificate of completion of a training program to choose between job applicants. These credentials are not always good measures of productivity differences.) Both placement efforts and selection on the basis of credentials make it easier for a trainee than his nontrained counterpart to find employment, even though there are no productivity differences between them.

Indeed, there is some circumstantial evidence to support the "sheepskin" and "placement" hypotheses. A number of studies have observed that earnings and employment differences between trainees and their control group (nontrainee counterparts) begin to disappear with time.[19] Such a reduction in benefits over time is what one would expect to observe if the earnings and employment differences were attributable to placement efforts and the effect of certification, rather than to actual differences in productivity. Additional supportive evidence can be found in the data on earnings differences for persons with different levels of formal education. The earnings advantages for persons with more years of schooling remains throughout their lifetime. This implies that if training improved real earning capacity, as schooling does, then the gains would have endured.

Certainly these are plausible explanations for the observed dissipation of training benefits over time, but there are a number of alternative explanations which are consistent with real, initial differences in productivity. The trainees could have been given occupationally specific instruction, the benefits of which were ultimately wiped out by technological or other economic change, or with the passage of time the nontrainees could simply have received more on-the-job training from employers.[20] Until the placement and sheepskin hypotheses are tested more thoroughly, it seems prudent to examine all three statistics: wage rate, earnings, and employment changes. If these hypotheses are corroborated, however, training programs would prove to be an expensive method of providing placement services and sheepskins.

[18] Sewell, op. cit., p. 45. James N. Morgan and Martin David, "Education and Income," *Quarterly Journal of Economics*, August 1963, are even more rigid about the measurement of productivity gains, suggesting that the change in annualized wages is the appropriate statistic, i.e., the change in wages per hour multiplied by 2,000 hours. The implication is that anyone who works less than full time is involuntarily unemployed. Again, inadequate fiscal and monetary policies are responsible for this, and the estimate of the gains from training will be biased downward unless the suggested correction is made. Not all unemployment is involuntary, however, and since the value of leisure is not included in NNP, use of this measure may overestimate the change in full employment output.

[19] In a followup study in West Virginia 4 years after completion of training, Somers and McKechnie found an improvement in the employment of nontrainees relative to trainees. Gerald Somers and Graeme McKechnie, "Vocational Retraining Programs for the Unemployed," *Proceedings of the 20th Annual Winter Meeting*, Industrial Relations Research Association, Gerald Somers (ed.), 1967, p. 34. Two other studies using the same West Virginia sample found employment and earnings differentials were significantly narrowed in the second year after training. Harold Gibbard and Gerald Somers, "Government Retraining of the Unemployed in West Virginia," and Glen Cain and Ernst Stromsdorfer, "Retraining in West Virginia: An Economic Evaluation," *Retraining the Unemployed*, Gerald Somers (ed.), University of Wisconsin Press, Madison, 1968, pp. 88 and 322, respectively. Cain and Stromsdorfer make special mention of the extensive placement and job creation efforts of the program managers.

[20] The probability is small, but the control group could also have had an initial, systematic preference for leisure.

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It is important to note that whether or not the benefits decline with time, increases in employment unaccompanied by wage increases may still be an indication of improved productivity. If exposure to a manpower program has converted the trainee into a more reliable worker with a reduced incidence of absenteeism, this is surely an increase in human capital.

Social costs.—The best definition of the economic cost of a program is "the value of the output which could have been produced with the resources actually employed in training." [21] Thus, the wages of training and administrative personnel, the depreciation of capital equipment, and the value of the output which trainees could have been producing are all relevant social costs.[22]

Certain cost items are always awkward to treat: allocating joint costs, estimating the depreciation on capital equipment, and valuing payments in kind. (States are often permitted to pay in kind for their share of training program costs.) Solution to these problems usually involves some arbitrary decision. There are numerous, extended discussions of these issues elsewhere.[23]

Because of the heterogeneous clientele, training costs for individuals in the same program may differ widely. Few evaluations attempt to relate cost differentials to the socio-demographic characteristics of the trainees. Although this does not affect the estimates of the rate of return for an entire program, it is clearly a potentially serious source of error in estimates of rates of return by socio-demographic categories.

Private benefits and costs.—Benefits are usually measured by any increases in disposable income which the trainee receives during his lifetime and which result from exposure to training. Costs are measured by any disposable earnings forgone during the training. Again, these statistics have the weakness of omitting fringe benefits. However, a change in transfer payments represents real change in an individual's disposable income, and hence is counted.

Taxpayer or government benefits and costs.—Increases in taxes (all taxes—income, sales, property, Social Security) paid by the trainee plus any reduction in transfer payments (such as unemployment compensation and welfare) for which the trainee would have been eligible constitute the benefits to the government. Training expenses (any direct financial expenditures associated with the program, including training allowances to enrollees) and reductions in taxes paid by the trainee during the program comprise the costs.[24]

An illustrative example which calculates benefits and cost from the three points of view appears in Supplementary Materials, Section II.

Isolating the Impact of Training

The question "What is the impact of training?" is unanswerable. The efficacy of training varies with the characteristics of the individual trainee and the economic conditions confronting him when he leaves the program and enters the labor force. The relevant question is

[21] Einar Hardin and Michael Borus, *The Economic Benefits and Costs of Retraining*, D. C. Heath & Co., Massachusetts, 1971, p. 15.

[22] One thorny conceptual issue is whether transfer payments, such as support payments to trainees while they are enrolled in training, should be counted as a program cost. From society's point of view they should not be, because consumption benefits foregone by the individuals who finance these payments are gained by the trainees. Glen Cain and Robinson Hollister, "Evaluating Manpower Programs for the Disadvantaged," in G. G. Somers and W. D. Wood (ed.), op. cit. p. 138.

[23] See Stromsdorfer op. cit., pp. 50–61 for a brief discussion.

[24] Hardin, op. cit., p. 103.

"What is the impact of training on whom, where, and under what circumstances?"

The local unemployment rate is perhaps the most obvious of the variables which could influence the participants' economic situation, and its effect must be distinguished from that of training. Discrimination in the labor market on the basis of race, age, and sex has been well documented. Since the intensity of such discrimination is likely to vary with geographic location, it may be desirable to control for region (North, South, etc., as well as urban and rural). Level of education and prior labor force experience are important determinants of earning capacity. Marital status and number of dependents are indications of obligations and responsibilities, and can often serve as a proxy for the motivation of the participants. In general, in order to isolate and measure those changes in the enrollees' economic situation attributable to training alone, the study design must control for those demographic characteristics and external influences which have an impact on labor force experience. Sampling is expensive, however, and analysis of some interesting socio-demographic groups is often thwarted by the limited number of observed cases.

Control group.—A crucial element in the design of any study is the control group: optimally, a group identical to the enrollees in every characteristic except exposure to training. The control group is a reference point, and the difference between its situation and that of the trainees in the posttraining period is used to measure the effect of training. Proper statistical procedure dictates that both the control group and the trainees be randomly selected from the population of interest.

Very few evaluations can comply with this requirement. Evaluations usually are made on a *post facto* basis, and reconstruction of a satisfactory control group is often impossible. Because control groups were inappropriate some evaluations of pre-1966 MDTA programs are suspected of overestimating the effectiveness of training. Prior to this date program administrators engaged in creaming—using intelligence and aptitude tests to select the most capable and easily marketable applicants.[25] Thus, a control group drawn from the remaining applicants was likely to be composed of less able people.

An unknown bias may be introduced if the control is drawn from nonapplicants. Eligibles who do not apply for training may be less intelligent or less motivated. On the other hand, they may be more self-reliant and independent, and feel that they do not need the assistance of a manpower program to extricate themselves from their current situation. Clearly, picking a control group is hazardous and should be undertaken with considerable care, since the results of a study are quire sensitive to its composition.

All too frequently, evaluations have used the enrollees themselves as the control group, using the change in the trainees' economic situation as the measure of benefits.[26] Such before-after comparisons can describe what happened to the trainees, but they cannot identify the cause of any changes. In particular, the effect of training cannot be

[25] Since 1966 this practice has been largely discontinued. At that time Congress amended MDTA, stipulating that 65 percent of enrollees be disadvantaged. "Manpower Report of the President," 1969, p. 78.

[26] For example, Olympus Research Corp., "Total Impact Evaluation of Manpower Programs in Four Cities," August 1971, one of the most comprehensive of the manpower evaluations uses before-after comparisons rather than a control group.

estimated because there are no untrained persons in the sample. Some enrollees are likely to be experiencing unusual, transitory economic reverses, reverses from which they would have recovered without benefit of manpower training. Their pretraining earnings understate the earnings which they would have received in the absence of training. If these people are used as their own control group, the influence of training will be overestimated. The most telling indictment of before-after comparisons is, that they are unable to determine whether observed changes in the trainees' economic position are due to training or to some other external change (such as a general increase or decrease in the demand for labor.)[27]

Length of observation period.—The expense of engaging in a longitudinal study has severely limited the length of the observation period for most evaluations. Few studies track the participants for more than a year, and only one is available with a followup sample 4 years after the completion of training. The projection of benefits 10 years into the future is a standard and unnerving practice, and it is the rare program whose benefits are so apparent and whose payback period so short that such projections are unnecessary to estimate a positive rate of return. Since our confidence in the reliability of such projections has been somewhat shaken by the preliminary evidence that training benefits may diminish within a few years, evaluations with longer observation periods should be funded.

[27] See Sewell, op. cit., pp. 23–24, for an extended discussion of these points.

MANPOWER DEVELOPMENT AND TRAINING ACT

MDTA is the manpower program with which we have had the most experience (1.7 million trainees over the course of a decade). The characteristics of the enrollees vary widely, and training has been both institutional and on the job. It is a potential fount of information and has been studied extensively.

MDTA's very breadth, however, makes it an unwieldy subject, and precludes any simple, unqualified determination of its effectiveness. In the mid-1960's MDTA's focus was changed to concentrate on disadvantaged trainees, as opposed to those who were considered easily employable. Unfortunately, only three of the benefit-cost analyses were conducted after this change in structure. The most recent of these (Olympus Research) is weakened by its reliance on before-after comparisons. (Refer to table 2.) Of the other two, Sewell's sample is

TABLE 2.—COST-BENEFIT ESTIMATES FROM SELECTED STUDIES OF MDTA

Name of study	Time period [1]	Location	Experimental group [2]	Control group	Internal social rate of return [3] (percent)
1. Main	1965–66	Nationwide	MDTA graduates and dropouts.	Unemployed relatives or neighbors.	15.9
2. Hardin and Borus	1962–65	Michigan	MDTA enrollees	MDTA applicants	14.7
3. Muir, et al.	1963–65	Nationwide judgment sample	MDTA graduates	Before-after comparison	
a. Institutional					54.0
b. On-the-job					56.0
4. Stromsdorfer	1959–63	West Virginia.	ARA and State program graduates.	Unemployed workers	138.0
5. Sewell	1965–67	North Carolina.	MDTA graduates: disadvantaged rural workers.	Nontrainee applicants	
a. Institutional					6.3
b. On-the-job					49.0
6. Olympus research	1969–70	Boston, Denver, San Francisco and Oakland.	Enrollees in MDTA and other training programs.	Before-after comparisons	(4)
7. Smith	1967–68	Nationwide	MDTA graduates	Simulation based on before-after comparisons	12.2

[1] The time period includes the training period and the followup observation period.
[2] In general these studies examine groups in addition to those for which rates of return are given. Main, Hardin and Borus, Stromsdorfer, and Smith examine institutional training only.
[3] These rates of return are based on an assumed benefit duration of 10 years.
[4] Rates of return were not available for this study.

Source: Modified from Ernst Stromsdorfer, Review and Synthesis, p. 99. The citations for the evaluations are: (1) Earl D. Main, "A Nationwide Evaluation of MDTA Institutional Job Training," Journal of Human Resources, spring 1968. (2) Einar Hardin and Michael E. Borus, Economic Benefits and Costs of Retraining, D.C. Heath & Co., 1971. (3) Allan H. Muir, et al., Cost Effectiveness Analysis of On-the-Job and Institutional Training Courses, Planning Research Corp., 1967. (4) Ernst W. Stromsdorfer, "Determinants of Economic Success in Retraining the Unemployed," Journal of Human Resources, spring 1968. (5) David O. Sewell, Training the Poor, Industrial Relations Center, Queen's University, 1971. (6) Olympus Research Corp., Total Impact Evaluation of Manpower Programs in Four Cities, August 1971. (7) Ralph E. Smith, An Analysis of he Efficiency and Equity of Manpower Programs, unpublished Ph. D. thesis, Georgetown University, September 1970.

relatively small, drawn entirely from a rural setting, and almost exclusively black, while Smith's study simulates control group earnings and employment. One of the most scientifically designed studies (Stromsdorfer) is so out of date that it treats trainees from MDTA's precursor, the Area Redevelopment Act, and the sample is composed of rural, white Appalachians. Of the nationwide studies, Smith's is the most reliable. Main's control group was developed in a rather unorthodox fashion, and Muir et al.'s results are suspect because of the use of unmodified before-after comparisons.

The studies by Stromsdorfer, Hardin and Borus, and Sewell are technically the most thorough and sophisticated of the evaluations, but each has important limitations. All are area studies, but given the diversity of the country, it seems impossible to develop an intensive evaluation whose results are uniformly applicable throughout the Nation. Only Hardin and Borus include urban as well as rural participants. Sewell alone is able to distinguish between the impact of on-the-job and institutional training, and Sewell's is the only sample composed primarily of disadvantaged persons.

It is encouraging that, with the exception of Sewell's estimate of the rate of return to institutional training, all of the studies indicate positive and large social rates of return. (The change in earnings due to training is used to measure benefits.) It is quite unlikely that such consistency would have been obtained if MDTA were not achieving some success. However, it is worth noting that if the benefits are assumed to last only 5 years instead of 10, then the Sewell, Main, Hardin and Borus, and Smith analyses would generate unacceptably low rates of return.[1] In light of some preliminary evidence that earnings differentials may decline within a few years after training, it seems premature to celebrate MDTA's achievements.

The rates of return in table 2 are estimates for entire programs, and as such do not reveal the very different impact that training has on various socio-economic groups. Since we consider Sewell's study to be the best piece of analysis for distinguishing such effects, the discussion is organized around it, with additional evidence (supporting and contradictory) drawn from the other evaluations.[2]

Institutional vs. On-the-Job Training

One of the most important findings in Sewell's study is that on-the-job training seems to be a good deal more successful than institutional training. The results are given in table 3. The entries in the table are estimates of the increase in trainees' weekly earnings, hours worked per week, and wage rate relative to nontrainees. These estimates were made after controlling for the influence of other variables

[1] The Hardin and Borus and Sewell institutional estimates are negative under this assumption.

[2] Sewell's study is exceptional for two reasons. Aside from having a good control group and including the major demographic variables in the regression equations, training was conducted in a labor market area which was close to full employment. Thus, the influence of inadequate demand was probably never present. Secondly his is the only study which incorporates directly into the regression a motivation variable, defined as a measure of the satisfaction which the individual derives from overcoming obstacles by his own efforts (Sewell, p. 118). Other evaluations caution that some omitted variable may be responsible for the benefits which have been attributed to training. The most likely candidate has always been individual motivation, and by direct inclusion of a measure for this effect Sewell has overcome a longstanding concern.

The study's most prominent weaknesses stem from its exclusively rural setting and almost entirely black sample population. There is, therefore, no opportunity to estimate differential impacts of training by race or in an urban environment. The fact that the county-of-residence variable showed both statistical and practical significance indicates that it was an important proxy for something, perhaps differential employment opportunities or industrial structures. It is a little disturbing that this variable was not fully exploited.

such as age, race, education, and motivation. (See note 2 to table 3 for the list of all the variables included in the analysis.) For example, after controlling for other variables, it is estimated that training increased the weekly earnings of female on-the-job trainees by $14.50.

TABLE 3.—EFFECT OF TRAINING ON WEEKLY EARNINGS, EMPLOYMENT, AND WAGES BY SEX AND TYPE OF TRAINING

	Males [1]			Females [1]		
	Weekly earnings (dollars)	Hours worked per week	Wage rate (cents)	Weekly earnings (dollars)	Hours worked per week	Wage rate (cents)
All OJT trainees	[2] 7.4	([3])	19.2	14.5	7.7	17.6
Completers	9.5	([3])	25.4	17.0	10.7	2.4
Dropouts	([3])	([3])	([3])	11.4	([3])	([3])
All institutional trainees	8.3	([3])	22.9	([3])	([3])	([3])
Completers	8.5	([3])	25.0	([3])	([3])	([3])
Dropouts	([3])	([3])	([3])	([3])	([3])	−50.9

[1] There were 287 males and 157 females in the sample.

[2] The statistics are the partial regression coefficients on the training status variable. The other independent variables in the equation are education, age, race, physical handicap, labor force experience, county of residence, mobility (number of States lived in since entering the labor force), and motivation (a test score indicating the individual's need to achieve). The coefficients are interpreted as the improvement in a trainee's status relative to a nontrainee after controlling for all the other variables. For example, male OJT trainees earned $7.40 more per week than nontrainees. All coefficients are statistically significant at the 1 percent level, i.e., there is only 1 chance in a hundred that training has no impact and that the true coefficient is zero.

[3] The estimate was not statistically significant at the 5 percent level. That is, at this level of confidence there is not sufficient evidence to conclude that training had any effect.

Source: David O. Sewell, Training the Poor, Industrial Relations Center, Queen's University, 1971.

On-the-job training led to a significant increase in the weekly earnings of both males and females, while only the earnings of male trainees were influenced by institutional training. Male earnings responded about equally to both types of training. However, the higher cost of institutional training (about twice that of on-the-job training) resulted in on-the-job training being a much better investment for men as well as women.[3] Even if one assumes that the benefits endured for only 5 years, the internal social rates of return[4] for on-the-job training were very respectable: 57 percent for the women and 17 percent for the men. The difference in rates of return by sex is due to the fact that female nontrainees worked less relative to their trainee counterparts than male nontrainees relative to theirs. The rate of return on the institutional training of men was negative if a 5-year benefit life was assumed, and only 11 percent for a 10-year life. The ineffectiveness of institutional training for women is discussed later.

Clearly, this conclusion could have major implications for the structure of manpower programs. Historically more than twice as many people have been exposed to MDTA institutional courses than to on-the-job training. If Sewell's results are true in general, then this pattern should be reversed. Unfortunately, sufficient information is not available to infer that these findings are generally true. Sewell's sample is relatively small, almost exclusively black, and entirely rural. Projecting the results from this study to an urban training program

[3] On-the-job training is not necessarily universally cheaper than institutional training. The cost estimates by Muir et al. indicate just the reverse.

[4] See page 22 for the definition of the internal rate of return. The term "social" rate of return is used here because it is the social benefits (measured by the increase in earnings at full employment which are attributable to training) and social costs which are being discounted.

would be a precarious leap into the dark.[5] Additional evidence is sparse. There are only two other studies which compare MDTA on-the-job and institutional training, and even their authors show a lack of enthusiasm for their conclusions.[6] MDTA program statistics on posttraining employment experience tend to support Sewell: 86 percent of MDTA on-the-job training graduates were employed 6 months after completing their training as opposed to 74 percent of the institutional trainees. (See table 1.)

Although the evidence which we have examined supports the widely held belief that on-the-job training is superior to institutional training, this evidence is neither extensive nor conclusive. There is an obvious difference between the two groups of trainees: on-the-job trainees have already been placed in vacancies and are employed. Thus some of the observed employment and earnings differences may be due to placement effects and not to the greater effectiveness of on-the-job training as a technique. There may be significant personal differences between on-the-job and institutional trainees. In the absence of an experimental design in which enrollees are randomly assigned to the two training methods, a differential impact cannot be identified. The issue is so important that research to resolve the problem soon seems imperative.

Additional Evidence on Institutional Training

The other studies in table 2 which employ control groups (Main, Hardin and Borus, and Stromsdorfer) treat only institutional training, so they cannot be drawn upon to evaluate the differential effectiveness of on-the-job and institutional programs. Their estimates of the impact of institutional training on weekly earnings tend to be somewhat larger than Sewell's. For all trainees the estimates are: Main, $7.87 to $9.60; Stromsdorfer, $10; Hardin and Borus, $18.77; Sewell, $5.90. For males alone Stromsdorfer's estimate is $10.26, while Sewell's is $8.30. (Women in the West Virginia study received no earnings benefits from institutional training. See the next section.)

In contrast to Sewell, where all of the earnings changes were accounted for by wage increases (except for female on-the-job trainees), Main found no statistically significant wage changes, all of the benefits being explained by employment increases (11 to 22 percent for those seeking full-time employment). The West Virginia trainees experienced employment increases in the same range (14 to 18 percent). Hardin and Borus do not present their results in a comparable manner, but it is likely that employment increases also account for some portion of the reported earnings benefits since the Michigan trainees had significant reductions in unemployment compensation. Like Sewell, Main found no earnings changes for drop-

[5] Knowledge of whether training has a differential impact on enrollees in urban and rural locations would seem quite useful to the Labor Department in allocating program funds. A priori, the wider market the more diversified industrial structure, and the higher turnover in an urban area suggests that trainees there might enjoy an advantage. We were unable to find any treatment of this question in the literature. Many studies have included an urban-rural variable, but none has interacted it with training. An urban-rural dummy variable functions only to explain traditional differences between the level of wages and earnings in urban and rural areas, providing no insight into the relative effectiveness of training in the two regions. It may very well be that there is no differential impact even if rural trainees are confronted with more limited opportunities. Their newly acquired skills may increase their mobility, and permit them to migrate more easily. Nonetheless, the question warrants consideration.

[6] The results in Edward C. Prescott, "Analysis of MDTA Institutional and OJT Data Tapes for 1968," Wharton EFA, Philadelphia, April 1971, support the superiority of on-the-job training, but Muir et al. find just the opposite. Both authors caution that their work is not definitive, and suggest more intensive research.

outs, but dropouts in West Virginia had improvements in both earnings and employment relative to nontrainees.

A number of factors may account for the discrepancies in the benefit estimates. The studies were conducted at different times, in different locations, with unequal sample sizes. The samples were drawn from populations with markedly different characteristics, and it is possible that the interaction of training with these characteristics generated the observed differences. There is some suspicion that this is not the case, however, and that omitted variables (creaming in the selection of applicants, abnormal placement efforts on behalf of the trainees, and motivation [7]) are at least partly responsible for the benefits attributed to training in the earlier studies. Follow-up samples of the West Virginia trainees revealed that earnings differentials between trainees and nontrainees declined after a few years, suggesting that placement may account for some of the initial benefits.[8] Hardin and Borus also think that creaming and placement efforts may have influenced their results.[9] The West Virginia participants have been described as fiercely proud and independent, indicating that the results may not be fully replicable elsewhere. We believe that, of the group, Sewell's is the most accurate estimate of training benefits for the disadvantaged, the estimates from the other studies being high.

Institutional Training for Women

The ineffectiveness of institutional training for women found by the North Carolina study is supported by the results in West Virginia. Female trainees had no significant earnings benefits relative to nontrainees, while males displayed earnings and employment increases sufficient to generate an estimated 61 percent internal social rate of return.[10] In attempting to explain some of the unsuccessful results of institutional training for women, a number of authors have expressed a rather jaundiced view of the motivations of women, particularly welfare recipients, who enroll in these programs. They suggest that the women are attracted by a diversion from their daily routine, particularly if child-care facilities and/or training allowances are provided. If they benefit from the program, fine; if not, they have incurred little, if any, opportunity costs.

Opponents of this view contend that the cause lies elsewhere, primarily in the occupations for which enrollees are trained: low-paying, unattractive jobs. Often the trainees' expectations are exaggerated, and they easily become discouraged when confronted with the realities of the labor market. If they are not welfare recipients, the incremental income from such jobs is reduced sharply by work expenses and child-care costs. If they are already receiving welfare, increased earnings may be offset by large reductions in welfare benefits. If placement and follow-up counseling services are perfunctory, these women soon leave the labor force.

There is anecdotal ammunition enough to supplement the sparse scientific information and supply the warring camps well into the night. Undoubtedly, both sets of factors have contributed to the inauspicious results at one time or another. Historically the Labor Department has maintained a policy of training enrollees in occupations for which a large number of vacancies has been reported. On the

[7] See footnote 2 for a discussion of motivation.
[8] See Introduction, footnote 19.
[9] Hardin and Borus, op. cit., pp. 21–22.
[10] Stromsdorfer, "Review and Synthesis," pp. 142–143.

face of it this seems eminently sensible. Unfortunately, the vacancies are often a reflection of high turnover rates due to low wages and adverse working conditions. Trainees placed in such positions are likely to respond in a predictably unfavorable manner.

This problem does not lend itself to facile solutions. If the Labor Department was to undertake a drastic restructuring of its programs and to train persons primarily for more highly skilled jobs in expanding industries, it would severely increase the cost of manpower programs, and society would have to be prepared to commit sizable additional resources for this purpose.

The evidence in the North Carolina study lends support to the hypothesis that there is a fundamental difference between women who enroll in institutional and on-the-job training. Sewell thinks that the choice of occupations for the institutional trainees also contributed to the result; all but one of them were trained as nurse's aides. However, examination of average, after training, annual earnings level for female on-the-job trainees ($1,857) [11] shows they were not placed in highly skilled executive positions either. They, nonetheless, remained in the labor market, and experienced relatively large increases in employment and wages. One cannot help but be struck by the interview responses of the institutional trainees:

> Our interviews overwhelmingly revealed that most of these trainees were neither working nor looking for work before or after they took the training courses. A few clearly regarded this training as a kind of home science extension course: one said her nurse's aide training helped in her occasional excursions into midwifery. This training may therefore have raised the nonmarket incomes of female . . . clients, and it is clear that there may also have been third-party benefits from these nurse's aide courses. Nevertheless, the alleged purpose of MDTA courses is to provide skills which the individual can use in employment. If our regression results contain a moral in this regard, it is that on-the-job training holds more promise than institutional training as a method of raising the earnings levels of women in the poverty population, because one can be more certain that a woman who undertakes on-the-job training is committed to the labor force.[12]

Impact of Training by Socio-Demographic Characteristics

One dominant theme emerges from the MDTA evaluations: disadvantaged persons [13] are able to derive benefits from training programs. This finding appears in studies of dissimilar sample populations enrolled in programs with markedly different structures in a variety of regions at different time periods. In fact, there is evidence that enrollees with certain disadvantages (low educational levels, long durations of unemployment, and low levels of pretraining earnings) experience larger gains from training than those who are less hampered in the labor market.

The evidence regarding the size of the benefits to the disadvantaged is mixed, however. Smith's simulation, based on a large, nationwide sample, indicates that the benefits are positive but small. Estimates of impressive improvement for the disadvantaged stem primarily from smaller, well-controlled, and technically sophisticated area studies. This makes interpretation difficult, for while one is loath to rely solely on a simulation, it is precarious to generalize from the area studies.

Education.—Sewell found that no matter what the measure of benefits (increase in earnings, employment, or wage rate) on-the-job

[11] Sewell, op. cit., p. 103.
[12] Ibid., p. 74.
[13] See Introduction, footnote 2 for the definition of disadvantaged.

training had a greater impact on those with 0–8 years of education than on high school dropouts.[14] The results from institutional training are not quite so consistent, but estimates of the training coefficients for those with less than a high school education are generally larger than those for all institutional trainees. These results are supported by the Michigan study where additional years of prior schooling decreased the influence of training on earnings. In West Virginia Stromsdorfer uncovered no improvement in either earnings or employment for high school graduates while the internal social rate of return for those with a grade school education was 98.9 percent, and for high school dropouts, 152.9 percent. For those cities where the education-training interaction entered the equation, the Four City Study found that the effect of training on wage increases became smaller with increased prior education.[15]

Duration of unemployment.—Both Stromsdorfer and Sewell found evidence that persons with a history of extensive unemployment prior to enrolling responded more to training than those who had been employed or who had been unemployed for shorter periods. Stromsdorfer estimated a 150.7 percent internal social rate of return from training those with more than 6 months' unemployment, while no other labor force-training interaction proved statistically significant. In North Carolina on-the-job training increased the earnings of the long-term unemployed more than other trainees, and institutional training influenced their wage rates more.

Race.—Surprisingly, the differential effect of training by racial origin has not been well investigated. Hardin and Borus provide the sole evidence on MDTA which is based on a control group comparison. They conclude that, regardless of sex, whites benefit more from training than nonwhites, but that all race-sex subsets exhibited internal rates of return of at least 150 percent (assuming a benefit life of only 5 years).[16] However, treating the same data from the taxpayer's viewpoint revealed that the Government earned a much higher rate of return from investing in training for nonwhites. Again assuming that the benefits last only 5 years, the rates of return were: nonwhite women, 144 percent; nonwhite men, 131 percent; white men, 37 percent; and white women, 12 percent. The reversal of the rankings by race was due to the significant reduction in welfare payments to nonwhites in the posttraining period Thus while white trainees made larger contributions to net national product, nonwhites were responsible for larger increments in tax collections and savings in transfer payments.

In the three cities where race was treated as an explanatory variable, the Four City Study found that white trainees had larger wage

[14] Although no regression was run for high school graduates, the training coefficients on earnings and employment for dropouts exceed those in the equations for all on-the-job trainees indicating that high school graduates were probably less affected by training.

[15] The Four City Study is exhaustive in the topics discussed and exhausting in its length, but its conclusions must be considered with the caveat that no control group was used and some of the analysis is unorthodox. For example, regression coefficients are presented with no indication of their statistical significance. It should be said in defense of this study that the statistics used to measure before-training earnings and employment performance are 3-year averages, and hence may be fairly reasonable estimates of the expected experience for the sample in the absence of training. Finally, because the experiences of enrollees from several training programs are lumped together in the analysis, results from this study are not strictly comparable with those of the other evaluations.

[16] These results are applicable only to those persons in the Michigan study who were enrolled in short training courses (60 to 200 hours). Enrollees in longer courses showed negative effects from training, a result which the authors were unable to interpret satisfactorily. Since the short course subsample was small (150), it is risky to rely heavily on these findings.

and income changes than blacks.[17] As in Michigan, however, blacks also benefited from training.

It cannot be emphasized enough that the explanation for the differential effect of training on the productivity of whites and nonwhites (if in fact one exists) is unknown. It could result from market discrimination which relegates blacks to inferior jobs, and prevents them from fully utilizing their training. It could be that blacks make a conscious decision to work fewer hours or at jobs with lower pay but higher nonmonetary benefits. It could be that other, inherent, personal differences (differences which are omitted from the analysis and are correlated with race) result in blacks not being able to benefit as much from exposure to training. It could be a combination of these demand and supply factors. However, two studies of income differentials between whites and nonwhites suggest that discrimination rather than motivation or inherent personal differences accounts for the inequality in earnings and employment.[18]

Age.—In both Sewell and the Four City Study, training had a greater impact on persons outside the group aged 21 to 43; however, the two studies have diametrically opposite results regarding the age group most likely to benefit. Sewell found that persons under 21 years of age experienced no benefits from training, but those over 43 had larger benefits than other trainees. The Four City Study concluded that youths seem to gain more from training than other age groups. However, before-after comparisons involving youths are particularly vulnerable to criticism because of the normally rapid rate of growth of earnings during the early years of employment. The confusion regarding which age group benefits most from training is compounded when the West Virginia results are examined: only the 31 to 45 age group showed a significant rate of return (129.3 percent).

Pretraining wage level.—One of the most interesting findings in the Four City Study is that in each city pretraining earnings and wage rates show a strong, negative correlation with the posttraining change in earnings and wages. This means that those trainees with the lowest earnings and wages prior to training experienced the largest increases in both absolute and percentage terms.[19]

The disadvantaged as a group.—In the evidence on the differential effect of training by particular demographic characteristics there is the suggestion that disadvantaged persons benefit more from training than those less restricted. Smith examined the disadvantaged as a group relative to the nondisadvantaged, and found just the opposite.

[17] The controlled-for variables differ from city to city but include number of dependents, length of time in training, work experience, age, sex, education, a proxy for the national unemployment rate, household head, and occupation trained for.

[18] Teh-wei Hu, Maw Lin Lee, and Ernst W. Stromsdorfer, "A Cost-Effectiveness Analysis of Vocational Education," Institute for Research on Human Resources, Pennsylvania State University, University Park, Pa., 1969, pp. 132–212, and Stephan Michaelson "Equating Racial Incomes: On the Efficacy of Employment and Education Policies," working paper, The Brookings Institution, November 1967. Hu et al. conducted a 6-year longitudinal study of high school graduates. The study controlled for intelligence and motivation, making it likely that the effects of discrimination were isolated.

[19] The consistency of this correlation in all four cities is impressive. There are, however, two points which tend to diminish its raw implications. The first is that this is only a two-way correlation, with no control for other variables. If a multiple regression format were used, the resulting relationship between pretraining earnings and wage rates and the posttraining changes in these variables might not be quite as dramatic. The second point is one that we have made previously in a slightly different form regarding a potential transitory, downward bias in the pretraining earnings of enrollees. Suppose that the expected change in permanent earnings due to training is independent of the level of these earnings. Then persons who, due to transitory reverses, had unusually low earnings in the pretraining period would have a higher probability of experiencing large changes in earnings in the posttraining period. Thus, a predictable statistical bias could be responsible for the observed correlation, rather than a differentially larger impact of training the lower a person's earnings.

Classifying the 109,000 individuals who completed MDTA institutional training in fiscal year 1967 as disadvantaged and nondisadvantaged, he found that the internal social rate of return for the disadvantaged trainees was only 3.5 percent (assuming 10 years of benefits), as opposed to 22.1 percent for the nondisadvantaged. Thus, although the disadvantaged in this group gained from training, the annual increase in their earnings due to training was absolutely quite small ($269), and relative to the cost of training small enough to render the investment economically inefficient.

This result is not conclusive because expected earnings in the absence of training were simulated, rather than based on control group observations.[20] Nonetheless, despite the volume of evidence from all the other studies, Smith's sample size is so large and his analysis so careful and convincing that it precludes an unequivocal conclusion that training is economically efficient for the disadvantaged.

In summary, the best that we can serve is a somewhat bland and distinctly unsatisfying fare: every study examined estimates an improvement in the economic position of the disadvantaged large enough to recoup the social cost incurred in training. At the very least, training generates a small, positive rate of return.

Indices of responsibility.—Numerous studies include independent variables which may be interpreted as proxies for degree of family responsibility (marital status, household head, number of dependents). These indices of responsibility usually bear the expected relationship to earnings and employment: people with more responsibilities are employed more and earn more. The question of interest is whether training has a differential impact which is dependent upon a person's level of responsibilities. Do people with more responsibilities benefit more from training? We found only two works which attempted to identify such an interaction. This limited evidence reveals no consistent relationship between the effectiveness of training and these indices. When the West Virginia sample was classified according to marital status, both married and single trainees exhibited very high internal social rates of return (168 percent and 106 percent, respectively, based on 10 years of benefits), but no statistical significance was found for the widowed, divorced, and separated group. In the Four City Study, the estimate of the influence that household head status had on the effectiveness of training was so small that it can be ignored. Similarly, number of dependents bore no consistent relationship to the impact of training on wage rates and earnings.[21]

Labor market conditions.—The measure which we adopted for the social benefits of training was the increase in net national product at full employment. The effectiveness of training is very likely to vary directly with the demand for labor in the local market. That is, training is likely to induce a larger difference between the earnings of trainees and nontrainees when unemployment is low than when it is high. An

[20] Estimates of pretraining average wage rate (adjusted for the national trend in money wages) and the expected percentage of time enrollees would have been employed during the training and posttraining periods if they had not undergone training were used to simulate earnings in the absence of training. The estimates of the expected percentage of time employed was based on past experience of race-age-sex-education cohorts, adjusted for the lower, national unemployment rate in the posttraining period. The difference between this simulated value for earnings in the absence of training and reported earnings for the graduates constituted the benefits attributable to training.

[21] The Four City Study used a stepwise regression technique. Household head entered the regressions for wage and earnings changes in only one city. Number of dependents was significant enough to be brought into the regressions for all four cities, but the random nature of the size and signs of the coefficients indicates no strong consistent relationship to training status.

analysis which estimates a uniform impact for training over the business cycle, and does not account for the interaction between the level of unemployment and training, probably overestimates the effect of training at high levels of unemployment and underestimates it at low levels.

None of the studies examined successfully included an interaction variable between the level of unemployment and training status, although all include an independent variable to account for some of the impact of fluctuating economic conditions.[22] Sewell, Main, and Smith may have avoided the problem, because their analyses were conducted in environments as close to full employment as we have had in recent years. No matter what the unemployment level nationally, however, local unemployment rates vary widely, and could produce differential impacts on the level of employment and earnings of trainees. Hence, it really is not safe to assume that these estimates of benefits are free of bias due to economic conditions.

There is some evidence that local employment conditions influence the success of MDTA programs, but it is rather puzzling evidence. Using data for 49 States for fiscal year 1968 (a period of very low unemployment nationally), Smith and Wertheimer found that a difference of 1 percent in State employment rates produced a 2¼ percent difference in the employment rate of current MDTA graduates.[23] This result is surprising, because the impact is almost negligible. The contrast with the influence that the recent recession seems to have had on MDTA's success is quite striking. While the national unemployment rate was rising from 4 percent in fiscal year 1970 to 5.7 percent in fiscal year 1971, MDTA successful placements [24] as a percentage of enrollments fell 10 percentage points (from 52 to 41 percent) and the same ratio for the on-the-job portion of MDTA fell 19 points (from 59 to 40 percent). Similarly, WIN and JOBS

[22] Both Hardin and Borus and Muir et al. attempted to estimate the effect of training under varying economic conditions. Muir et al. felt that their data were not sufficiently representative to warrant drawing any conclusion. Hardin and Borus were able to estimate coefficients on labor market variables entered as independent variables, but none of these survived as interactions with training status. Their conclusion that the benefits of training were not sensitive to variations in local economic conditions is too alien to our intuition to be accepted as a generality with passing equanimity. Stromsdorfer, Sewell, and Main included proxies to control for variations in economic structure and labor demand, but their estimated models do not specify an interaction term. Smith made a crude correction in his data for altered national economic conditions, but his analytic technique does not lend itself to anything so refined as an interaction with training status.

Striking corroboration for one's intuition is evidenced in the data from the Four City Study. In the equations explaining earnings and wage rate changes, a variable is included for the time of year that a participant left the training program. The national unemployment rate worsened progressively during the course of this study, rising from 3.6 percent in the fourth quarter of 1969 to 5.8 percent a year later. Without exception, in the three cities where the time variable entered the equation, the coefficients change from large and positive early in the period to large and negative later in the period. This means that persons who completed their training while economic activity was still brisk experienced much larger increases in wage rates and annual earnings than those who entered the labor market after it became sluggish. In Boston the estimated annual change in earnings due to training is over $1,000 greater for the initial completers than for the last group of graduates. In Oakland the difference is $220 and in San Francisco $760. The results are almost as consistent for dropouts. Unfortunately, the design of the study makes it impossible to determine whether the lower earnings of later graduates is simply a reflection of increased unemployment throughout the economy (which would have depressed the incomes of nontrainees, as well), or is due to an interaction effect (which would reduce the differential between trainees and nontrainees).

[23] Ralph E. Smith and Richard F. Wertheimer, *Evaluating Efficiency in a Decentralized System*, The Urban Institute, October 27, 1971, page 7. 2.25 is the value of the partial regression coefficient on the State employment rate. It is statistically significant at the 1 percent level. The other independent variables are percentage of trainees who were nonwhite and percentage over 44 years of age. A similar relationship undoubtedly exists at the national level, but the coefficient on the national employment rate will not necessarily have the same value. Although a linear structure proved to be the best fit, Smith and Wertheimer believe that this was due to data limitations, and that in fact the change in the employment rate of training program graduates is likely to get larger and larger the closer the State's employment rate is to one.

[24] Employed 6 months after initial placement.

are much more sensitive to economic conditions than Smith and Wertheimer's estimate for MDTA. Of course, the percentage of disadvantaged in these two programs is much higher than that in MDTA.

Assessing the Results

In the context of society's commitment to the work ethic, there is clearly some encouraging evidence in the MDTA studies. Disadvantaged and low-income persons have responded to training and have become more self-sustaining. It is important that we retain our perspective, however. The results which we have been examining pertain to training programs during the last decade. Although the absolute number of trainees during that period was quite large, it is likely to be miniscule in comparison with the number for whom training would be specified if manpower programs were adopted as a mainstay of income-maintenance policy. No one knows whether a massive training effort for the low-income and welfare population will generate similar outcomes.

First of all, increased enrollments will make it difficult to duplicate the quality of past training programs. Instructors are a scarce resource, and attempts to hire more of them may increase the per capita cost of training. Selecting the positions for which participants should be trained is already an uncertain task, and the risk is multiplied as the program grows. Judgment errors will occur, creating excess supplies of some occupational skills.

Thus far MDTA trainees have comprised a negligible proportion of the labor force, and the additional competitive pressure which they have exerted on wage rates has probably been small. A much more ambitious program of training for the low-income population would encounter increasing difficulties in getting graduates absorbed into the private sector. The most sanguine economic model (one which assumes flexible wage rates) predicts that employment is available for the trainees but at somewhat lower wage rates. This, of course, would reduce the rate of return from training. In a less competitive world, encumbered with institutional restrictions, placement in the private sector becomes more doubtful, and a supporting program of public employment may prove necessary.

All MDTA trainees in the past have been volunteers. If compulsory training were instituted for particular categories of welfare recipients, changes in the motivation of the "participant" population, if not the qualifications, could be anticipated. Even if future trainees were equally capable, resistance to mandatory training could produce results altogether different from those which have been witnessed previously. In addition, if women responsible for young children are included in the mandated population, the problem of insufficient child care arrangements will have to be confronted for these women to be able to remain active in the labor force.

The recent inflationary pressures have produced a reluctance on the part of the Administration to vigorously pursue a policy of full-employment demand. In the long run, training programs should reduce some of the skill shortages which help to fan inflation, but it would be naive to expect them to eliminate the problem. Given the sensitivity of the success of manpower programs to the level of economic activity, a continuation of current macroeconomic policies will make it impossible to realize the training benefits estimated.

We have couched our discussion in terms of increases in earnings due to training and the rates of return on investment. These are important measures of program success and economic efficiency, but their significance can be overemphasized. The economic efficiency of MDTA is only a secondary objective; reducing the incidence of poverty is its primary concern. Although some research results suggest that the gains in earnings have been large relative to costs, the earning increases have not been large by conventional, social standards. It is sobering to note that the average posttraining annual earnings for the trainees in Sewell's sample was $2,406, a gain of $433 over nontrainees, but still $471 below the poverty line for this group.[25] In terms of absolute and percentage gain in annual earnings as well as the rate of return on investment, female on-the-job trainees were the most successful of the North Carolina participants. Yet their posttraining annual earnings averaged only $1,857. The MDTA trainees in the Four City Study did somewhat better, averaging $3,100 in posttraining annual earnings. But this was still over $800 below the relevant poverty line.[26] If child care and work expenses have to be financed from these earnings, there is not much left for the amenities of life. Training does reduce the poverty gap, but continued income supplementation is likely to be necessary for the graduates.

[25] Based on the 1967 poverty line and a weighted average of the sample by number of dependents and sex. Sewell, op. cit., pp. 102–103.
[26] The 1970 poverty line for a nonfarm family of four was $3,968.

PROGRAMS FOR YOUTH

There are two separate manpower programs which serve only youths under 22 years of age: NYC and Job Corps. NYC has three components for two different constituencies (the in-school and summer programs for students and the out-of-school program for dropouts), and can be discussed as two distinct programs. While the Job Corps was designed to provide enrollees with skills which are transferable to the labor market and will increase their employability, the NYC program until recently has provided work experience opportunities with very little occupational training. The objectives were to encourage the youths to finish high school, to provide them with earning opportunities, to improve their self-discipline and work orientation, and to overcome some of the obstacles confronting them in the labor market. Responding to criticism of this program design as a mere aging vat for teenagers, which did nothing to augment the effects of the normal maturation process on participants' employability, the Department of Labor restructured the out-of-school program in 1970, placing emphasis on supportive services, remedial education, and skill training.[1] An effort is also being made to make the in-school and summer programs more sensitive to the students' individual needs and to increase the skill content of their work experience. If these attempts to improve the efficiency and effectiveness of the NYC prove successful, the results of past evaluations may no longer be valid.

Although these programs have been the subject of numerous research projects, only three benefit-cost analyses have been done, one for each program. The economic results are mixed, varying widely by sex, ethnicity, and years of education. Despite the generally high level of technical competence of these studies, their results deserve only tenative acceptance. Reservations arise about the appropriateness of the control group in each of the evaluations and about the models used to estimate the benefits. Conclusions about the educational impact of NYC are uniformly discouraging, suggesting that the program may be badly conceived as a solution to the dropout problem. In some instances it seems to have significantly reduced the probability of high school graduation.

Neighborhood Youth Corps—In-School and Summer

Social Economic Returns

Somers and Stromsdorfer have conducted an extensive study of a nationwide sample of NYC participants.[2] The authors fit two quite different models to the data, but used only one of these to make their estimates of the program's impact on employment and earnings. This is significant because the two models give very different results. The model used by the authors treats all NYC participants equally, regardless of their length of stay in the program, and estimates the impact of NYC membership on post-high school earnings and employment experience. The second model is identical with the first except

[1] Because of the lack of facilities in rural areas, all of the redesigned projects are in urban areas.

[2] Gerald G. Somers and Ernst W. Stromsdrofer, *A Cost-Effectiveness Study of the In-School and Summer Neighborhood Youth Corps*, Industrial Relations Research Institute, University of Wisconsin, Madison, July 1970. The period of observation covers the interval from July 1965, to October 1969. Participants had to be graduated from the NYC by the end of June 1967.

that the length of time that a participant was enrolled in the NYC is substituted for NYC membership. This second model can be used to estimate the influence that each additional month of program participation had on post-high school earnings and employment. (The authors used this model only to estimate the optimal length of stay in the program.) The first model indicates that the program had a significant impact on earnings and employment. The second model implies that the post-high school benefits were trivial. The authors' estimates are reproduced in table 4 and discussed below. These results are contrasted later with estimates which we generated from the second model.

The entries in columns 3 through 7 of table 4 are the estimates of the differences between the earnings, taxes paid, months unemployed, and months voluntarily out of the labor force of the NYC participants and those of the control group. Only those estimates which were statistically significant are given. The estimates were made after controlling for the influence of other variables.[3] The entries are interpreted as the differences between the average experiences of the two groups during the average number of months that they were eligible for the labor force. For example, for the total sample the estimate of the increase in pretax earnings due to NYC participation is $831 during a period of 18.56 months, or $45 per month.

TABLE 4.—POST-HIGH SCHOOL ECONOMIC BENEFITS OF THE IN-SCHOOL AND SUMMER NYC[1]

Sample group[2]	Average months eligible for the civilian labor force	Average social rate of return[3] (percent)	Total before tax earnings	Total after tax earnings	Total Federal income and social security taxes	Months unemployed	Months voluntarily out of the labor force
Total (n=676)	18.56	[4] 90.1	[4] $831	[4] $702	----	----	[4] −2.30
Male (n=311)	14.04	[5] 137.0	[5] 1,171	[5] 876	----	[5] 0.79	----
Female (n=365)	22.32	----	----	----	----	[5] 3.11	[6] −5.12
White (n=398)	20.01	[4] 109.6	[4] 1,013	[4] 794	----	----	[4] −3.06
Negro (n=166)	12.19	[6] 170.2	[6] 1,579	[6] 1,186	[6] $286	[6] −3.09	----
White male (n=202)	14.98	----	----	[4] 445	----	----	----
Negro male (n=57)	9.75	[5] 144.6	[5] 1,182	[5] 1,094	[7] 271	[6] −6.89	----
White female (n=196)	25.27	----	----	----	----	----	[4] −4.56
Negro female (n=109)	13.47	[5] 137.2	[5] 1,217	----	[4] 255	[4] −2.00	----

[1] The entries in cols. 3 through 7 are the partial regression coefficients on training (a 0, 1 dummy variable indicating control group or enrollee status). Only statistically significant values are presented. The column headings define the dependent variables, and a tabled value is interpreted as the change in a dependent variable as a result of participating in the NYC. For instance, for the total sample the effect of NYC participation was to increase pretax earnings by $831 during the average, post-high school period of eligibility for the labor force (18.56 months), holding all other variables constant. The other independent variables in the regression for the total sample are age, age squared, year and quarter the individual left high school, months of work experience during high school, marital status, father's education in years, sex, ethnic origin, population of area of residence, and a discriminant function which attempts to correct for remaining personal and social differences between the NYC and control groups (such as motivation or native intelligence). Identical regressions were run on the subsamples, except that the variable of subdivision is removed as an independent variable; e.g., if separate regressions are run on males and females, sex cannot be a variable in the equation. Two independent variables indicative of responsibility (household head and the product of household head and number of dependents) are included in the regressions on after-tax earnings and taxes.

[2] The value of n is the sample size for each group.

[3] The internal social rate of return is based on benefits experienced during the period of eligibility for the labor force, not on a future projection.

[4] Significant at the 0.05 level.

[5] Significant at the 0.10 level.

[6] Significant at the 0.01 level.

[7] Significant at the 0.109 level.

Note.—All tests are 2-tailed. For an explanation of level of significance, see the notes to table 3.

Source: Somers and Stromsdorfer, op. cit.

[3] See the explanatory notes to the table for a full description of the models.

The post-high school increase in earnings experienced by former NYC participants was due entirely to their increased labor force participation relative to the control group (2.3 fewer months voluntarily out of the labor force). The fact that there was no difference in wage rates indicates that employers did not consider the former trainees to be any more productive per hour than their control group counterparts. Although there was no difference in the duration of unemployment experienced by the trainees and nontrainees, the unemployment *rate* for the trainees was reduced, since they spent more months in the labor force. The estimated internal social rate of return (90.1 percent) is especially impressive because it was based only on observed benefits (differential earnings during the post-high school period of eligibility for the labor force), not on a projection of future benefits.[4]

Subdivision of the sample by sex gives the result that only males had statistically significant earnings increases. The perverse tendency of NYC males toward slightly more unemployment in the post-high school period than their control group counterparts was offset by the fact that the male trainees worked 7.1 more hours per week. Although NYC females participated in the labor force considerably longer then their control groups (5.12 more months), they reaped no earnings benefits, because much of the additional time was dissipated searching for work (3.11 more months unemployed), and because, once employed, they worked 6.1 fewer hours per week.

The NYC was not ineffectual in training all women, however; black females did benefit. They had large increases in before-tax earnings ($90 per month of eligibility for the labor force) and reduced unemployment spans (2 months less than nontrainees). White females do not seem to have benefited. Although they were in the labor force 4.56 more months than their control group and experienced similar amounts of unemployment, they worked 11.3 fewer hours per week, and had no increase in earnings.

There is a consistent pattern in the data indicating that blacks benefited more than whites. Whether one examines the race or the race-sex subgroups, one finds that black trainees had larger earnings differentials relative to their counterparts than whites, and they also had significant reductions in unemployment, while whites did not.[5] Again, the reasons for differential effects by race are unknown. One possible explanation is that the placement efforts of NYC officials overcame discrimination barriers which would otherwise have confronted black youths.

Benefits by Program Component

Participants can be enrolled in any of three program combinations: in-school only, summer only, or both in-school and summer. No

[4] Since the length of time that the sample members were eligible for the labor force varied, it seems unusual to use *total* post-high school earnings as the dependent variable. Although the inclusion of months of eligibility as an independent variable may have corrected for some or all of the earnings differences due to this variation, one is still left with the uneasy feeling that an incorrect specification may have been used, and that something like earnings averaged over the period that an individual was eligible for the labor force should have been substituted.

[5] It is difficult to explain why in the regression on the white subsample, NYC participants experienced significant earnings increases, but when the sample is further divided by sex, neither white males nor females seem to have benefited. Two possibilities come to mind. One is that when the sample of whites was subdivided by sex, there were too few observations relative to the amount of variance in the data to obtain a statistically significant coefficient. Since the subsamples have 200 observations, this appears unlikely. Alternatively, the race-sex models are not strictly comparable to the regressions on race alone. In order to make them comparable one would have to add a sex-training interaction variable to the race equations. If this were done for the whites, multicollinearity between the sex-training and training variables might prevent estimation of significant coefficients on either term.

matter what measure of economic benefit was employed (social, private, or government), Somers and Stromsdorfer found no evidence that the summer-only enrollees benefited relative to their control group. This was true for the entire sample and for each sex and ethnic subgroup. Since the program period is so short, it is doubtful that it had delayed benefits beyond the observation period. If the summer-only program is to be justified, it will have to be on grounds other than its impact on participants' future earning capacities.[6]

The estimated internal social rate of return was quite high for both of the other components (132.6 percent for the in-school only and 138.2 percent for the in-school and summer). However, before-tax earnings increases were statistically more significant and occurred for more subgroups of the sample for the in-school only component.[7] This anomaly is a bit difficult to explain, but Somers and Stromsdorfer think that it may be due to the fact that the control group had a longer period of labor force eligibility than the combined in-school and summer participants.[8] It appears that on efficiency grounds alone the in-school-only program is the best investment.

Private and Government Benefits

The pattern of after-tax earnings differentials accruing to NYC participants mirrors the before-tax benefits with two exceptions: white males earned significant after-tax benefits ($30 per month of labor force eligibility) and black females received no after-tax benefits. Both of these deviations in the pattern are difficult to explain, but are probably due to different family structures (and hence tax rates) for trainees and nontrainees in these subgroups.

Among the NYC trainees only blacks increased their tax contributions. These did not cover the Federal Government's outlays for training, but, as was explained earlier, this was not an objective of the program.

Some Qualifications

Although the control group was chosen from the same high school and socio-economic strata as the trainees, application of a standard statistical test[9] indicated that the two groups probably were not drawn from identical populations. The authors attempted to correct for differences between the two groups by introducing a variable called a discriminant function.[10] Nonetheless, they warn that some of the estimated benefits of NYC membership may be due to these personal differences.

A variable for population of the economic area is included in the analysis to control for possible differences in wage levels, industrial structures, and employment opportunities. However, no variable is

[6] Some studies have reported reduced police contacts for NYC participants, but the findings are preliminary. *The Neighborhood Youth Corps: A Review of Research*, Manpower Research Monograph No. 13, Washington, D.C., 1970, p. 14.

[7] The male, female, and Negro subsamples showed statistically significant increases, and whites were almost significant at the 10-percent level. For the combined in-school and summer component whites were the only subgroup with statistically significant benefits, and this at the 10-percent level only.

[8] Somers and Stromsdorfer found no significant differences between the characteristics of participants in the two components, but it is conceivable that more able persons participated in the in-school-only program and tried to locate better paying jobs during the summer than were available through the NYC. NYC participants are paid the minimum wage.

[9] The Chow test.

[10] See note 1 to table 4 for a brief description of this function or Somers and Stromsdorfer, op. cit., 132–142 for a detailed discussion. The discriminant function was introduced to reduce any systematic bias between the control and NYC groups resulting from self-selection.

introduced to control for variations in local labor market unemployment conditions. Since the national unemployment rate was quite low in 1965 (4.5 percent) and fell throughout the period covered by the study to 3.5 percent in 1969, this does not appear to be a serious omission.

Estimates from the Alternative Model

An important inconsistency exists in the analysis, and it cast suspicion on all of the benefit estimates. Somers and Stromsdorfer generated a model other than the one used to estimate the benefits presented in table 4. They substituted the number of months that an enrollee participated in the NYC for the variable which merely indicated trainee or nontrainee status, and reestimated the equations. According to this modified model, there were virtually no post-high school earnings increases attributable to NYC participation. For an enrollee who remained in the NYC for the optimal amount of time (12.9 months), the expected before-tax earnings increase was slightly less than $1 a month.[11] This implies a negative social rate of return, and is in striking contrast to the earlier estimated gain of $45 per month and a 90.1 percent return.

We can only speculate on the reasons for these widely divergent results, and offer the following explanation. Suppose that the more able and ambitious enrollees left school and the program relatively early to enter the labor force, and they earned the largest income increases, while those who remained in the program longest did so partly because they were unable to locate positions preferable to those available through NYC.[12] Once the latter group entered the labor force, it continued to have employment difficulties. If this was the case, and if, as the authors suspect, the initial model does not control adequately for individual differences in motivation and ability, then the benefits resulting from these character differences may have been inadvertently attributed to NYC training status. That is, the estimate of the impact of training is upward biased, reflecting not just the earning capacity of participants, but also the impact of differences in their native abilities. Since the NYC variable in the second model is more sensitive, distinguishing as it does between differential lengths of exposure to the program, we are inclined to believe that it is the more accurate specification of the problem, and that the resulting coefficient is a better measure of the influence of NYC on the human capital of the participants.

The data for this study are very complete and unlikely to be duplicated in the near future We recommend that additional tests be performed on these data to resolve the inconsistency in the estimates of economic benefits.

[11] Benefits increase with length of stay in the program, but each additional month of NYC participation produces a smaller incremental increase in post-high school earnings. Maximum benefits occur at 12.9 months. Estimated incremental benefits are negative for each month of participation thereafter. The coefficients are statistically significant at the 1 percent level.

[12] NYC participants must be paid the legal minimum wage, and may not work more than 15 hours per week during the school year. Some enrollees may have chafed under these restrictions; one study of NYC found that the principal reason for dropping out of the program was the need for a better job. *The Neighborhood Youth Corps: A Review of Research,* Manpower Research Monograph. No. 13, Washington, D.C., 1970, p. 11. An evaluation of NYC in Cincinnati and Detroit revealed that new enrollees were very optimistic that the program would improve their future employment opportunities. By the time of their last interview many had become disillusioned, and most thought the experience would influence their employability only moderately. Ibid, p. 35. These attitudinal findings are consistent with our conjecture that ambitious participants may become restless, and drop out of the program.

Educational Impact of NYC

Although the focus of this review is the effect of manpower programs on the earning capacity of individuals, the success of NYC in reducing the dropout rate and extending participants' years of schooling is a relevant consideration because of the well-documented correlation between education and income. Since the primary legislative rationale for NYC is to encourage continued school attendance, a number of evaluations have been attempted. None are optimistic about the efficacy of the program. In fact, several authors believe that the program is fundamentally unsound, having found evidence that it actually reduces the probability of high school graduation.

In a study of NYC projects in Cincinnati and Detroit, Robin concluded that the program was not influential in reducing the dropout rate, increasing the educational aspirations of enrollees, their studiousness, or their scholastic achievement.[13] He found that the work experience distracted students who already had low grades. They further reduced the minimal amount of time which they devoted to their studies. Somers and Stromsdorfer estimated that the program had no impact either on the probability of high school graduation or on years of schooling completed.[14] For those who graduated from high school, the program increased the probability of attending college or of pursuing some postsecondary schooling. However, these probabilities were reduced to practical insignificance when length of enrollment in the NYC was substituted for the dummy variable indicating trainee or nontrainee status.

Reducing the opportunity cost of schooling for disadvantaged students by making part-time employment available to them is a commendable, humanitarian objective. The determinants of the dropout rate are complex, however, and it appears that the NYC is too simplistic a mechanism to constitute an effective attack upon the problem.

Neighborhood Youth Corps—Out-of-School

No analysis based on a national sample exists for the out-of-school program. However, Borus *et al.* have done a benefit-cost analysis of the program in five cities in Indiana.[15] The analysis is excellent technically, but there are some important limitations. The localized nature of the study makes generalization hazardous.[16] The authors cautioned that they were unable to introduce controls for differences in motivation or intelligence. Again, differences in these characteristics may have been responsible for some of the observed benefits which were attributed to NYC participation. Cost estimates were based

[13] Gerald D. Robin, *An Assessment of the In-School Neighborhood Youth Corps Projects in Cincinnati and Detroit, With Special Reference to Summer-Only and Year-Round Enrollees*, National Analysts, Inc., Philadelphia, February 1969, p. ii.

[14] A multiple regression model was used with controls for NYC participation, age, income per capita per family, urban-rural residence, frequency with which respondent dropped out of school, father's education, ethnicity, sex, and the discriminant function (discussed in notes to table 4). The results varied by sex, race, and sex-race subsample, with most of the estimates of educational impact being statistically insignificant. Among the subgroups with significant coefficients there are as many with negative impacts as positive. If length of time enrolled in NYC is used instead of trainee or nontrainee status, NYC has a statistically significant but practically unimportant impact upon years of schooling and the probability of high school graduation.

[15] Michael E. Borus, John P. Brennan, and Sidney Rosen, "A Benefit-Cost Analysis of the Neighborhood Youth Corps: the Out-of-School Program in Indiana," *The Journal of Human Resources*, Spring, 1970, pp. 139–159.

[16] Forty percent of the out-of-school enrollment is in rural areas (June 1971), and, of course, no inferences can be drawn about them. We have no way of knowing whether the findings are duplicated in other urban areas.

on average costs in each city, because data were not available to distinguish training expenses by demographic characteristic.

The most serious reservation about this study arises from the composition of the control group. Although the control and NYC groups displayed virtually identical distribution of personal characteristics,[17] administration or self-selection biases easily could have been present in the population from which the control group was drawn. The control group members came from a population which was eligible and had applied for the out-of-school program during the same period as the trainees, but they either were placed on a waiting list and never assigned a slot, could not be located for assignment, or failed to report when assigned. Any one of these events could be associated with greater or lesser ability or ambition. These qualifications not withstanding, we present the results.

As in Somers and Stromsdorfer, two formats were used for the estimated equations: one with a variable indicating trainee or nontrainee status, the other with a variable for hours of participation in the program. Total earnings in 1967 was the dependent variable.[18] In contrast to Somers and Stromsdorfer, the coefficient on the trainee status variable was statistically insignificant, but that on hours of participation was significant at better than the 5 percent level. Each additional hour of training increased annual earnings by $.33, but the benefits varied by sex and level of education. Participants averaged 520 hours in the program. Expected annual benefits for this amount of training were $173.

Impact by Demographic Characteristic

Sex.—At the mean educational level (10 years) females benefited by only $.16 annually for every hour of exposure to training, while males benefited by $1.08. Except under liberal assumptions about costs, the discount rate, and the duration of benefits, these expected increases in earnings for women were not sufficient to generate social benefit-cost ratios greater than 1. Men, on the other hand, exhibited ratios greater than 1 even under rather conservative assumptions.[19]

Education.—High school dropouts showed higher benefit-cost ratios than graduates. The ratios were greatest for those with 9 or 10 years of schooling and declined thereafter. This suggests that considerable success can be had by encouraging early school dropouts to enroll in the program.

[17] Sex, education, age, marital status, family size, language used at home, and race.

[18] The authors went to some pains to include in the analysis all those persons who were eligible for the labor force during all of 1967, whether or not they had any earnings. Thus, they obtained a full year of post-program *observations*. They even included some sample members who were jailed, because they "felt that their incarcerations accurately reflected their future post-program labor market experience." Borus et al., op. cit., p. 143. Other than training status the independent variables were sex, education (in years), education squared, age, age squared, marital status, family size, language used at home, a control for the date a participant left the program, and city of residence. Interactions between hours of training and sex, education, and education squared were also included. There was no explicit control for local labor market economic conditions, but it is likely that city of residence served as an adequate proxy for this. In each case the coefficient on city of residence was statistically insignificant, due probably to the close-to-full-employment conditions in each city throughout 1967. Under these circumstances the addition of an interaction variable between training and local labor market conditions probably would not have influenced the estimates.

[19] If the output produced during training is counted as a benefit, a 5 percent discount rate is employed, and benefits are assumed to endure for 10 years, then women with 9 or 10 years of schooling have ratios of 1.2, but those with 8, 11, or 12 years of education have ratios less than 1. Under these same assumptions the ratios for men range from 2.9 to 4.0. Making any one of these assumptions more conservative reduces the benefit-cost ratios for women at every educational level to less than one. On the other hand, the ratios for men at every educational level continue to exceed one even if all assumptions are simultaneously made more conservative (10 percent discount rate, 5 years of benefits, and output produced by trainees is valueless).

Race.—The inability of the authors to obtain a statistically significant coefficient on a race-training interaction variable suggests that whites and nonwhites benefited equally from the program.

Comment

Given the attendant qualifications, Borus *et al.* does not constitute a definitive study of the out-of-school program. The evidence from this study does suggest that the program is having success in helping school dropouts to adjust in the labor market. Some of the findings have already been implemented: enrollment in the out-of-school program is now limited to 16 and 17 year olds, a policy consistent with the result that early high school dropouts benefit most from the training. However, if the economic inefficiency of NYC out-of-school training for women in Indiana proves to be universal, structural changes in the program will have to be made to meet the needs of females.

Job Corps

In separate analyses of the same national sample, Cain and Resource Management Corporation (RMC) have produced two unrefined, preliminary studies of the effectiveness of the Job Corps.[20] The control groups for these studies are so suspect and the observation periods so short that the results are unreliable.[21] However, if the estimated benefits and costs prove accurate, the Job Corps will have to be classified as economically inefficient.

The earnings gains estimated by RMC would never produce benefit-cost ratios greater than 1, no matter how far into the future the gains were projected. Cain concluded that the Job Corps was economically efficient and that its rate of return was at least equal to that of some other Government investments. However, using Cain's most optimistic estimate of benefits, earnings differences between the corpsmen and the control group would have to persist for 24 years to generate an internal social rate of return of 5 percent, and 42 years adopting his most likely estimate.[22] These estimates suggest a marginally efficient project at best. For the Job Corps to achieve a 5 percent internal social rate of return within 10 years, the corpsmen would have to earn $454 more annually than the control group, more than twice the $203 differential which is Cain's most likely estimate.

Both studies used the gross differences between the employment, unemployment, and wage rates of the corpsmen and the control group as measures of the Job Corps' impact.[23] Cain did not attempt to isolate the effect of training from the influence of other variables; RMC

[20] Glen G. Cain, *Benefit/Cost Estimates for Job Corps*, Institute for Research on Poverty, the University of Wisconsin, Madison, September 1968, and Harry R. Woltman and William W. Walton. *Evaluation of the War on Poverty; the Feasibility of Benefit-Cost Analysis of Manpower Programs*, Resource Management Corporation, prepared for the U.S. General Accounting Office. March 1968.

[21] Neither Cain nor RMC were responsible for the design of the sample, and hence cannot be blamed for the inadequate nature of the control groups. Cain used "no-shows" for his control group (persons who were accepted for but never participated in the Job Corps), while RMC used no-shows and early dropouts (enrollees who were exposed for less than 3 months). We have already discussed the unknown nature of the biases which a control group composed of no-shows can introduce into the analysis. (See the critique of the NYC out-of-school program.) Identical problems arise from the use of dropouts.

[22] It is a distortion to characterize the first estimate as "optimistic." It is based on the assumption that the corpsmen will never experience any unemployment in the years following training; that is, they will work 40 hours a week, 52 weeks a year. The most likely estimate assumes an 18.5 percent unemployment rate.

[23] Cain also projected an earnings gain for the corpsmen based on educational gains (for which they were tested) and a relationship estimated by Hanoch between earnings and education. The benefit-cost ratios based on educational gains were generally lower than those estimated from the wage gains.

controlled only for sex.[24] Cain found that 6 months after separation from the program, ex-corpsmen earned $.12 more per hour than the control group. Apparently this wage gain did not persist. When RMC examined the sample 18 months after termination, the wage differential had declined and was no longer statistically significant even at the 10 percent level. RMC found no apparent impact on rates of employment or unemployment.

The use of gross comparisons is hazardous in the absence of a randomized experimental design, and these studies were far from approximating such a design.[25] Without a carefully chosen, representative control group, this technique may lead to spurious results. At best such comparisions add little to knowledge of program effectiveness. It is true that the gross results are not encouraging, but they were not adjusted for the possible influence of personal differences or variations in local labor market conditions.[26] The normal instability of young persons' earnings increases the unreliability of the projections. Finally, the observations are on 1966 trainees, only the second year of Job Corps' operation, when it was still experiencing growing pains, and may not reflect its current effectiveness. It would be injudicious to assess the Job Corps on the basis of these studies.

[24] RMC's control group of early dropouts had demographic characteristics quite similar to those of the corpsmen, but the control group used by Cain differed significantly from the corpsmen in several respects. The no-shows had slightly more schooling, 8 percent fewer males, 7 percent fewer whites, an unemployment rate at the start of the program of 66 percent as compared with 35 percent for the corpsmen, and were somewhat older. One would expect these differences to create opposing biases, the net effect being unknown. If Cain had controlled for the influence of these differences in demographic characteristics on earnings gains (say, by introducing the characteristics as independent variables in a regression), the poor match would not be so significant. But since no such controls were applied, his estimate of the benefits of Job Corps training are suspect. When making before-after comparisons of employment and wage rates, RMC segmented the sample by a number of personal characteristics. However, they controlled only for sex when estimating improvements relative to the control group.

[25] If persons from the target population have been randomly assigned to the control and training groups, then differences in the variables of interest (wage rates, earnings, etc.) between the two groups can be attributed (with specifiable statistical confidence) to exposure to the training.

[26] Unemployment nationally was quite low (approximately 3.8 percent) during the postprogram observation period, but, again, local variations could have affected the results.

WORK INCENTIVE PROGRAM

Since the participants in WIN are all members of families receiving public assistance (AFDC), its effectiveness is of particular interest to us. Unfortunately, this interest is largely frustrated by the poor caliber of research on WIN. This situation is all the more appalling since quite a few studies have been conducted and, presumably, quite a bit of money spent. No analysis has been conducted which uses a control group.[1] (As always, this is an important omission; turnover rates on AFDC are high, and in order to isolate the effect of exposure to WIN, it is essential to know whether WIN enrollees are any more successful in the labor market than nonparticipating AFDC recipients.) There have been no longitudinal studies of the labor force experience of WIN participants. Since post-training earnings and employment data are so scanty, the statistic most often relied upon to evaluate WIN's effectiveness is the percentage of successful placements (the percentage of those leaving the program who are placed in jobs and are still employed at the follow-up time, 3 to 6 months after placement).

There is no need to dwell on the multiple deficiencies of such a gross and insensitive statistic for determining the impact of WIN on the long-range economic situation of trainees. It controls neither for personal nor environmental variables, gives no insight into income increases or welfare receipt decreases relative to a control group, and provides no basis for comparing benefits to costs.[1a]

It may be that these objections are overkill and that the unsound features of WIN combined with the unfavorable economic conditions of the past few years made success an impossibility and rendered deference to conventional scientific methodology unnecessary. If the entire bureaucracy were to conspire against them, AFDC recipients could not be confronted with fewer work incentives than the current system provides. Earnings over $30 a month reduce AFDC benefits by two-thirds.[2] An AFDC father is subject to the additional restriction that, if he is employed more than 100 hours a month (regardless of the amount of his earnings), his family becomes ineligible for public assistance. These disincentives are reinforced by a reduction of in-kind benefits as earnings increase The prices which a low-

[1] A longitudinal study of WIN which uses a control group was published too late for critical examination in this paper: Ronald E. Fine, et al., *Final Report, AFDC Employment and Referral Guidelines*, Institute for Interdisciplinary Studies, Minneapolis, June 1972. The authors concluded that WIN services did not increase the earnings or employment of the trainees.

[1a] Even the placement data is unreliable. Upon leaving WIN a terminee is classified as a successful placement, a dropout, or other (a dropout with legitimate cause). In the past, the WIN staff has not reported information on terminees who locate employment on their own in a uniform manner, some having been classified as placed and some as other. Officially such persons are supposed to be identified as conditionally placed and a follow-up interview conducted 3 to 6 months later, but the staff has not always complied with this regulation. Some researchers believe that a large number of the dropouts may also have located employment themselves and were termed dropouts because it was too expensive to trace them after they stopped reporting to their WIN assignments. It is clear that a conscientious longitudinal study of the destiny of WIN enrollees is sorely needed.

[2] In the seven States that pay only a percentage of needs deficit (needs less countable income), the tax rate is two-thirds of the specified percentage. In States which impose a maximum on the public assistance payment, the tax rate is zero if the maximum is applicable.

income family must pay for food stamps, school lunches and public housing all increase with income. Medicaid coverage ceases entirely should a family earn enough to escape the welfare rolls.[3] (Recipients however are reimbursed for some or all work expenses, so such expenses do not reduce the incentive to work.)

If the stick is large enough, the size of the carrot may be irrelevant. Under the Social Security Act, if a person is referred to WIN, but refuses to participate, the uncooperative person may not be included as a family member for purposes of computing the family's welfare payment. Also, instead of issuing a check to the family for its public assistance benefits, the welfare agency must engage in vendor payments (direct payments to merchants to cover the majority of the family's expenses). However, the penalty does not apply to mothers who volunteer for WIN. Since most of WIN's clientele are volunteer mothers, the sanctions are largely a fiction.

Fathers cannot avoid the penalty in this manner. Examining the effectiveness of sanctions on fathers in Los Angeles and Denver, the GAO found that the financial penalty for noncompliance was not large in Los Angeles ($19 per month), and although substantial in Denver (about $50 per month), it was frequently not imposed, primarily because of the administrative expense of making vendor payments and a reluctance to impose hardship on the family.[4] Further, because a family is ineligible for AFDC once a father is employed more than 100 hours a month, family income was often reduced less if the father refused to participate in WIN and accepted the penalty than if he accepted employment. In light of the small work incentives, the virtual absence of penalties for noncompliance, the high national unemployment rate (especially among the relatively low skilled), and the documented reluctance of employers to hire AFDC recipients,[5] it is remarkable that WIN's placement rate is as high as it is.

[3] This is potentially a very large and abrupt benefit loss because, as long as a family receives even $1 of public assistance, it pays no share of the cost of covered services. In 24 States families which are ineligible for public assistance qualify for medicaid if their income is sufficiently low. However, coverage begins only after an initial outlay for medical services, and in some States the benefits are less extensive than those available to public assistance recipients.

[4] No family in Denver had sustained a financial penalty although 94 males had refused to participate in WIN. Penalties had been imposed in about half of the Los Angeles cases. Vendor payments were rare in both cities. Comptroller General of the United States, *Problems in Accomplishing Objectives of the Work Incentive Program*, General Accounting Office, Washington, D.C., September, 1971, pp. 33–4. In hearings before this subcommittee Ferrell C. Sparks, manager of the Atlanta, Georgia Metropolitan Manpower Center, stated that "it becomes apparent to some welfare recipients that lack of effort to seek employment does not jeopardize their welfare check." *Hearings before the Subcommittee on Fiscal Policy of the Joint Economic Committee*, Atlanta, Georgia, June 7, 1972.

[5] Placement efforts for WIN enrollees are conducted through the Employment Service. In hearings before this subcommittee Ferrell C. Sparks reviewed the problems confronting the Employment Service due to its tarnished reputation in the private sector. "The characteristics of these welfare recipients . . . typify the average job applicant who visits our office for job assistance. Our reputation with many employers of this area is one that suggests inability to provide them with suitable applicants to fill their openings. . . . Employers do not rely on our service to fill their 'good' openings, but will list their openings only after all other methods of recruitment have failed. We are considered to be a very poor source of recruitment of qualified applicants. . . . Employers have become more and more conscious of the fact that applicants sent to them from our office have many handicaps and problems which would be a liability to them if hired. Applicants who are skilled or semiskilled . . . often times can easily find employment themselves and do not desire or need our services." Ibid.

TABLE 5.—WIN STATISTICS—FROM ASSESSMENT THROUGH SUCCESSFUL COMPLETION (CUMULATIVE SINCE JULY 1968)

	As of Dec. 31, 1970	As of Apr. 30, 1972
AFDC recipients assessed	2,284,476	NA
Appropriate for referral (according to State welfare agencies)	512,056	NA
Referrals	[1] 398,222	NA
Enrollments	228,822	385,131
Terminations	119,660	257,283
Successful completions (on the job 3 to 6 months after placement)	23,691	61,500

[1] The Labor Department found approximately 135,000 or 34 percent of these referrals inappropriate and sent them back to the State welfare agencies. The rest of the gap between referrals and enrollments is accounted for by "no-shows" and intake holding.

Sources: (1) Services to AFDC Families, U.S. Department of Health, Education, and Welfare, Washington, D.C., July 1971. (2) Materials Related to H.R. 1: Work and Training Provisions, Committee on Finance, U.S. Senate, July 23, 1971. (3) WIN table 18, Cumulative Enrollment and Terminations, U.S. Department of Labor, Washington, D.C., June 1972.

Indeed, gross statistics on WIN are discouraging, undoubtedly a reflection of the above difficulties. After 2 years of operation only 1 percent of the assessed AFDC recipients had completed training and been employed for a minimum of 3 months. (See table 5.) However, this is a loaded statistic, which distorts one's perception of WIN. It is unjust to condemn WIN's efficacy as a training program on this basis when, in the combined judgment of the State welfare agencies and the Department of Labor, at least 83 percent of those assessed were inappropriate for referral to WIN.[6] It is hardly the fault of the training program that the goal of significantly reducing the AFDC rolls in a short time by training and transferring recipients to employment may be unrealistic and unrealizable.

While completely inadequate for benefit-cost purposes, the percentage of former WIN participants who held jobs at the time of followup is a more legitimate statistic than the percentage of those assessed who were placed. Successful completers have been a constant 20 percent of terminees until recently; during the period April 1971 to April 1972, they increased to 30 percent.[7] This increase runs counter to the trend in the rate of unemployment nationally (5.9 percent from April 1971 to April 1972, but only 4.2 percent from July 1968 through April 1971), an indication that WIN may have gained some experience in dealing with the specialized problems of its clientele and solved some of its internal difficulties. Dropouts without good cause continue to average 21 percent of terminees, however.

The comparable successful completion rate for MDTA has been considerably higher than that for WIN. It averaged 51 percent between 1963 and 1971. Of course, the client populations are quite different, and the comparison indicates nothing about the relative rates of return.

[6] Most of these persons were classified as unsuitable for the following reasons: illness, disability, or advanced age; remoteness from WIN projects; full-time student age 16–20; presence required in the home because of illness or incapacity of another member of the household or because of number or age of children; adequate child care arrangements unavailable; currently receiving other training or education.

[7] This increase is entirely accounted for by a reduction (from 59 to 49 percent) in the proportion of those who dropped out of WIN for legitimate reasons. The successful completions category is a lower bound for the number of former WIN participants who are employed 3 to 6 months after leaving the program. Dropouts from training and persons who terminate for legitimate reasons are not placed in followup status, and, hence, no information is collected on their post-program labor force experience. Undoubtedly, some of them also become employed.

WIN authorities have had their feet held to the fire because of the number of enrollees who have completed their training and are waiting to be placed in jobs. This number soared from 1,648 in May 1970 (1.8 percent of enrollment) to 9,621 in March 1972 (7.7 percent of enrollment). As a percentage of enrollment it has now stabilized, remaining between 7 and 8 percent throughout the fiscal year 1972. The percentage of enrollees awaiting job placement is very highly correlated with the national unemployment rate in the present and recent past. There is an estimated 2.9 percentage point increase in the percent of WIN enrollees awaiting job placement for every 1 percentage point increase in the national unemployment rate.[8] At current enrollment levels this amounts to an additional 3,000 people who have completed their training and are waiting for jobs, or approximately the number of trainees who successfully complete the program each month. Since it is beyond the capacity of the WIN authorities to increase the demand for labor in the local market, they probably have been subjected to some undeserved criticism.

WIN Placement and Dropout Patterns by Demographic Characteristics

Analytic Systems, Inc. (ASI) has produced an interesting disaggregation of the early WIN termination data (October 1968 through March 1970). Although nothing can be learned from these data regarding WIN's impact on trainees' earnings relative to a control group (because there was no control group), placement patterns and dropout rates provided a basis for tempering expectations about the potential self-sufficiency of the AFDC population under the present structure of work incentives.[9] Despite the fact that national unemployment rates during the observation period were the lowest in the last 15 years (3.6 percent), the highest placement rate achieved by any demographic subgroup was only 31 percent, and this was for males who (according to WIN staff assessments) had no serious barriers to employment at the time of enrollment.[10] A subjective determination of labor market barriers is suspect, of course, but at the very least these were the enrollees who were perceived to have the fewest inadequacies.

A few words of caution regarding these program data: although they constitute some of the best available information on WIN, they pertain only to its first 18 months of operation. The internal program changes implemented since then together with the altered economic situation may have affected the placement and dropout patterns. Secondly, there are three WIN termination categories: a dropout without good cause, a dropout with a legitimate reason, and a successful placement (em-

[8] Regressing the percent of enrollees in job entry holding (from May 1970 to March 1972) against the unemployment rate in the previous quarter enables one to explain 94 percent of the variance in job entry holding. The increase in the percentage of WIN enrollees waiting for jobs was estimated using this regression. It is difficult to use this data to estimate the impact of changes in the unemployment rate on WIN's placement rate. According to the Department of Labor, 85 percent of those conditionally placed in jobs successfully complete the program; that is, are employed 3 to 6 months later. Hence, fluctuating economic conditions have a potentially large effect on the placement rate.

[9] The sample was not random but ASI describes it as "representative of the program nationally." *Analysis of WIN Program Automated Termination Data*, Analytic Systems, Inc., Vienna, Va., November 1970. The sample was quite large (15,662), however, and constituted 26 percent of the population.

[10] The barriers were low education or lack of skill, being outside the prime age labor force, unstable family care arrangements, a record of conviction or garnishment, or health, personality, or transportation problems.

ployed 3 to 6 months after placement).[11] In the discussion that follows, we use the word "dropout" to refer to those who terminated without good cause. Finally, because the placement and dropout rates are classified and examined separately for each socio-demographic characteristic (as opposed to a multivariate analysis), it is very difficult to provide dependable explanations for differences in the rates.[12]

Sex.—ASI divided the sample by sex and examined the placement and dropout rate patterns by age, marital status, household head status, number of dependents, race, education, and past labor force experience. No matter what the variable of classification, almost without exception, women displayed lower placement rates, lower dropout rates, and higher rates of termination for legitimate reasons. This is an interesting and puzzling result, but in the absence of a multivariate analysis it is impossible to provide a satisfactory explanation. It is only conjecture, but the significantly lower dropout rate for women may reflect the fact that almost all female participants were volunteers. AFDC fathers were referred to WIN whether they wanted training or not, and hence they may have been less motivated. Although women may have been more motivated, they face more barriers to employment. Greater family care responsibilities, the frequent breakdown of child care arrangements, and fewer employment opportunities may account for the higher rate of legitimate termination and the lower rate of successful placement.

Indices of maturity and responsibility.—There is nothing that is counter-intuitive here and little discussion is necessary. Placement rates increased with age, falling off for those over age 54. Dropout rates were very high for trainees less than 18 years old (33 percent for males, 27 percent for females), and declined with age thereafter. The dropout rate for males 65 and over increased sharply.

Household heads had a placement rate almost twice that of nonheads and a significantly lower dropout rate than nonheads. The same was true for married, widowed, divorced, or separated males relative to those never married. The pattern of relative placement and dropout rates by marital status held for females, but the differences were somewhat smaller.

The placement rate increased and the dropout rate decreased with the number of dependents. Females with dependents had an increasing placement rate up to 3 dependents; for females with more than 3 dependents the rate was fairly stable (at about 20 percent). The leveling off of the rate for females with large families was undoubtedly related to child care problems. WIN had very poor success training and placing those with no dependents: a 9 percent placement rate and a 28 percent dropout rate.[13]

[11] A dropout without good cause is any person who refuses to participate in the program, cannot be located, or was administratively separated, e.g., dangerous conduct. The legitimate reasons for dropping out are referred in error, appeal accepted, returned to welfare, death, moved from the area, transportation problems, family care required, pregnancy, health, institutionalized, entered Armed Forces, entered full-time schooling, transferred to another program, and other.

[12] All of the differences discussed are statistically significant at the 1 percent level or better. However, in a format where only one variable is controlled, highly misleading results can occur because the variable of classification may be serving as a proxy for some other variable with which it is correlated. For example, suppose that most whites in the sample were age 25–45, while most nonwhites were less than 25 years old. Then observed differences in placement rates between whites and nonwhites may not have been due to ethnic differences at all but rather to differences in age. If whites and nonwhites in the same age bracket were examined, then one could have more faith that racial differences accounted for differential placement rates.

[13] These are either the teenage, out of school children of AFDC parents (in which case "no dependents" is probably a proxy for age) or pregnant women.

Race.—Placement rates for blacks and whites were identical at 21 percent, but other ethnic groups (primarily American Indians, Mexican Americans, Puerto Ricans, and Orientals) had only a 15 percent rate. A lack of local employment opportunities for Indians and a language barrier for Spanish-speaking persons are possible explanations for their lower rates of placement. There are no significant differences in the dropout rates of females by ethnic origin, but only 19 percent of the white males dropped out as compared to 28 percent of the nonwhite males.

Education.—A diploma effect is fairly evident in the placement data. Male high school graduates had a 30 percent placement rate, while male high school dropouts had only a 23 percent rate. The placement rate for female high school graduates was more than twice that for dropouts (27 percent versus 12 percent).

Additional years of schooling between the fourth and eleventh grades did not influence the placement rate. However, those with only the lowest levels of grade school education (1 to 4 years) had significantly lower placement rates (18 percent for the men and 10 percent for the women). It seems rather poignant that they also had the lowest dropout rates, an indication that they were not failing for lack of trying.

High school dropouts evidenced higher dropout rates from WIN than any other educational group, but this may be a misleading correlation. Fifty-nine percent of AFDC mothers who dropped out of high school are less than 30 years old. Hence, age rather than status as a high school dropout may be the better explainer of the dropout rate from WIN.

Employment experience.—Placement rates increased and dropout rates decreased with years of prior work experience.

Summary

Thus, the pattern of successful completions is generally consistent with one's intuition: WIN participants with characteristics which put them at a disadvantage in the labor market had more difficulty locating employment.[14] This is not to say that WIN was less effective in improving the employment prospects or increasing the earnings of this group. At the risk of being repetitious, there is no way to determine from these data whether WIN had any impact at all. A longitudinal study utilizing an appropriate control group is the *sine qua non* for such knowledge.

The pattern of dropout rates is not quite so uniform. Some groups which were at a disadvantage in the labor market (older workers, females, and those with only a few years of grade school education) did not quit the program without good cause, but a number of disadvantaged groups had relatively high rates of illegitimate attrition (nonwhites, younger enrollees, and those with little work experience). Enrollees with family responsibilities had both lower dropout rates and higher placement rates than those without such responsibilities.

WIN has been a small program relative to the size of and growth in the AFDC population. Since there is evidence that WIN authorities

[14] Additional supporting evidence for this comes from J. David Roessner, *Employment Contexts and Disadvantaged Workers*, Bureau of Social Science Research, Inc., Washington, D.C., November, 1971. "Those WIN enrollees who achieve placement status . . . are a heterogeneous group that exhibits few of the characteristic problems associated with disadvantaged persons . . ." Ibid, p. 192.

creamed in selecting enrollees,[15] the prospects for improved placement rates and for subsequent reductions in the welfare rolls by expanding a structurally unaltered program are not encouraging. The recent inclusion in the tax code of a tax credit to employers for 20 percent of the wages paid to WIN participants during their first year of employment should help a bit. But the limitations on this credit reduce its potential for increasing employment among welfare recipients. Twenty percent of wages are allowed as a credit up to a maximum of $25,000 per employer. (This is equivalent to the credit for only 25 full-time workers at a $2.50 hourly wage rate.) Thereafter the credit is cut in half to 10 percent, severely reducing the attractiveness of hiring WIN graduates. Also, a tax credit provides no incentive to tax-exempt institutions and government agencies; a direct wage subsidy would.

Although the tax credit is a welcome improvement in WIN, plans for rigid enforcement of the sanctions against dropouts without simultaneous increases in work incentives seem ill-considered. An enrollee can always sit through training, and then avoid employment by making himself sufficiently unattractive to a prospective employer. In the absence of financial inducements, resistance from those compelled to participate can be expected. Finally, reducing the large number of WIN participants who drop out for legitimate reasons is going to require improved labor market conditions, longer periods of training to provide greater skills, and solutions to participants' health, transportation, and family care problems, all of which may prove expensive.

[15] In May 1969, mothers enrolled or awaiting enrollment in WIN had considerably more schooling than all AFDC mothers.

[In percent]

	Enrolled in work or training	Awaiting enrollment	All AFDC mothers
High school graduates	41.4	44.2	16.3
1 to 3 years of high school	30.2	23.7	31.4

JOB OPPORTUNITIES IN THE BUSINESS SECTOR

The objective of the JOBS program is to place disadvantaged persons who need on-the-job training and supportive services in private industry jobs. The program is administered by the Manpower Administration in conjunction with the National Alliance of Businessmen (NAB), a nonprofit organization which encourages firms to participate in the program and assists them in the operation of the training.

JOBS was initiated during the very tight labor markets of the late 1960's, when the corporate sector was experiencing recruiting difficulties. The original goal was to place 500,000 disadvantaged persons in meaningful (that is, skilled) employment within 3 years. The program has both a contract and noncontract component. Contract employers are reimbursed for the extraordinary costs[1] involved in hiring and training disadvantaged persons. Noncontract employers agree to hire a specified number of disadvantaged but receive no subsidy. By the end of June 1970, NAB and the Department of Labor were reporting 494,000 trainees hired (with the noncontract component accounting for 74 percent of these) and a retention rate of 47 percent.

The JOBS program provided an excellent opportunity to test the contention that decentralized training conducted by employers is more efficient than centrally administered programs. The argument is that (1) firms are more aware of the skills required in their production process; and (2) there is no period of discontinuity between training and placement, a period in which some trainees may become discouraged and leave the labor market. A counterargument contends that this is a myopic view. It is to society's advantage to provide some general, transferable skills which make the worker more mobile and insulate him from technological or long-term unemployment. An individual employer has an incentive to provide general training only to the extent that it is necessary to perform the tasks specific to his production process.

There was an opportunity to test both views but it was missed. No controlled studies of the impact of JOBS on the employment and earnings of enrollees were conducted and the program data submitted by participating firms to NAB and the Labor Department are so unreliable as to be unanalyzable. Employers are supposed to provide NAB with a quarterly accounting of the number of persons hired under the program, the number currently in training, the number who have completed their training and have been retained, and the number terminated. For those terminated, employers are required to specify the type of termination (voluntary, discharged, laid-off) and the reason (unsatisfactory job performance, acceptance of other employment, excessive absenteeism, et cetera). When the GAO conducted an investigation of JOBS, it found not only that detailed information on trainees was not being reported, but also that the number of persons reported by NAB as hired by noncontract firms frequently

[1] Those costs over and above those associated with training nondisadvantaged persons for similar positions.

exceeded the number actually hired.[2] In a significant number of cases the reported information could not even be verified, because the noncontract employers maintained no records on the trainees.[3] The GAO discovered one instance where a noncontract employer had reported hiring 5,000 persons under JOBS who had been employees of the company prior to JOBS' inception. The number of disadvantaged reported as hired was further distorted because employers themselves frequently certified trainee applicants as disadvantaged rather than referring them to the Employment Service for this determination.[4] Consequently, a significant number of persons who were not disadvantaged were hired under both the contract and noncontract components.

The casual manner in which data was collected as well as more substantive problems in the JOBS program were fostered by the atmosphere in which the Labor Department negotiated and awarded contracts and monitored training operations. In order not to discourage employer participation in the program, contract negotiations were rushed through with only limited scrutiny of training cost estimates and the details of the manner in which training and supportive services were to be provided. Monitoring seems to have been perfunctory, at least in the early stages of the program.[5] A number of predictable problems developed. A significant number of employers in the GAO sample did not supply the supportive services which were stipulated in their contracts and for which they were reimbursed. Greenleigh Associates confirmed this finding in an evaluation conducted for the Labor Department.[6] There were instances of over-

[2] Detailed information had been submitted to NAB for only 44 percent of the trainees reportedly hired. In four of the five cities sampled by the GAO, the number of trainees reported as hired exceeded the actual number. Comptroller General of the United States, *Evaluation of Results and Administration of the Job Opportunities in the Business Sector (JOBS) Program in Five Cities*, Washington, D.C., March 24, 1971, pp. 15–16.

[3] NAB used the unverified statistics on JOBS accomplishments to mount a publicity and public relations campaign of considerable proportions. NAB's first annual report estimated that over 80 million viewers had been exposed to NAB commercials on national television and that an average of 500 news stories about the program appeared in newspapers and magazines every month.

[4] Such referrals were mandatory for contract employers but could not be required of noncontractors.

[5] The Labor Department established a schedule of visits by "contract service representatives," with frequency based on the size of the contract. "These representatives are not to function as contract compliance officers during postaward visits, but to aid contractors in program operation. Implicit in this activity, however, is the notification of the appropriate regionally based monitoring staff if any contract irregularities are uncovered." Arnold R. Weber, Assistant Secretary for Manpower, U.S. Department of Labor, in a letter to Senator Ralph Yarborough, printed in *The JOBS Program, Background Information*, Subcommittee on Employment, Manpower, and Poverty of the Committee on Labor and Public Welfare, United States Senate, U.S. Government Printing Office, Washington, D.C., April, 1970. Apparently, few were uncovered. In reading the representatives' reports regarding their visits to contractors the GAO found no reference to any of the substantive program operation difficulties which the GAO itself uncovered. According to the GAO, NAB exerted pressure on the Manpower Administration against monitoring the program operation too closely, it was deemphasized and only limited resources allocated to it. Field monitors were given inadequate guidance (to the extent of not getting any instructions in how to evaluate the quality of the program operation and effectiveness). Comptroller General of the United States, *Evaluation of JOBS*, pp. 73–76.

[6] "Although job coaching and counseling was a mandatory component fewer than 45 percent of the companies interviewed provided such service and, in many of these, the service that was provided was so inept that it was almost totally unproductive. It was apparent that there was a measurable relationship between inadequacy of the job coaching function and dropout and turnover rates." Greenleigh Associates, Inc., *The Job Opportunities in the Business Sector Program, An Evaluation of Impact in Ten Standard Metropolitan Statistical Areas*, New York, June, 1970, p. 117. Some employers, confronted by the GAO regarding nonfulfillment of their contracts, replied to the effect that ". . . in their opinion, it was not necessary to provide the services in the manner or to the extent required by the contracts." Comptroller General of the United States, *Evaluation of JOBS*, p. 65. Some small employers complained that they simply could not supply all the services required and simultaneously manage their business. This suggests that the capacity of employers to deliver the services was not fully evaluated before contracts were awarded.

payments which more conscientious monitoring could have prevented.[7] Most important, both the GAO and Greenleigh emphasized that many of the jobs filled under the program were positions traditionally held by low skilled and unskilled persons. The GAO estimated that 20 percent of the jobs offered were of this character. Greenleigh Associates were more critical:

> Most jobs held by JOBS employees fell into the following general categories: laborers, machine operators, maintenance helpers, and assemblers. Thus, most of the jobs pledged by employers were concentrated in occupations which traditionally have high turnover rates.[8]

As exposés and vehicles for initiating program improvements, the GAO and Greenleigh studies serve a useful function. However, they are often anecdotal. Although it undoubtedly is true that the program was subject to inadequate supervision, contract violations, instances of subversion of its objectives, and improper design of some elements, it is equally true that many firms were sincere in their efforts and committed to aiding the disadvantaged. Because of their design and the inadequate data, these studies cannot address the central issues of concern to us: Did the employment and earnings of disadvantaged persons increase as a result of the program? Did the benefits justify the expenditures involved? Is it possible that contract firms employed no additional disadvantaged persons, that the program has served merely as a subsidy to firms who have filled vacancies created by the attrition of some of their low-skilled employees with other low-skilled workers? As is clear by now, such questions are difficult, but then no systematic attempt was made to answer them with respect to the JOBS program.

The Labor Department has drawn a random sample of 12,000 from the Social Security earnings records of JOBS employees, comparing their earnings for 1966 (prior to the inception of JOBS) with those for 1968 (the program's first year of operation). The mean earnings of these workers increased from $1,499 to $2,592, a difference of $1,100 and a 73 percent change. The number reporting no earnings decreased by 90 percent, and those with earnings between $4,000 and $6,000 increased by 50 percent.[9] These are impressive gross figures and it would be hard to believe that the program did not account for a sizable portion of the gains in employment and earnings. Nonetheless, this is only a before-after comparison. The study of Social Security records had no control group, and did not correct for the influence of other variables. The observation period is decidedly short. If some of these workers were employed in uncovered occupations before entering JOBS, their earnings would not have been reported to Social Security, and the increase in earnings estimated from the Social Security file will exaggerate the true increase. Most importantly, the Social Security study provides no insight into the displacement problem, which plagues all training program evaluations and for which no solution is in sight.

[7] The GAO appears to have been a bit overcharitable in its characterization of most of the overpayments as caused by "misunderstandings of the billing procedures." Contractors were paid for the number of days that an employee was given on-the-job training. "The errors in the invoices were caused generally by the manner in which the contractors calculated the number of days that trainees actually worked. In some cases the contractors estimated the number of work days in the month, rather than determining from payroll records the number of days actually worked. In other cases the contractors kept no record of amounts previously claimed for the days a trainee worked and, as a result, claimed amounts in excess of the maximum amount allowable for the trainee. We also noted instances where the contractors continued to include amounts for trainees after they had terminated and for regular employees who were not trainees." Ibid., p. 71.

[8] Greenleigh Associates, Inc., op. cit., p. 66.

[9] Subcommittee on Employment, Manpower, and Poverty, op. cit., pp. 168–9.

As an initial step in this direction, it might have been useful to examine the composition of the labor force of participating firms. If the percentage of disadvantaged employed by them did not increase, the program's effectiveness would certainly be suspect.

Recommendations for Improving JOBS

The objective of the JOBS program is not just to place the disadvantaged in the kind of jobs they might have gotten anyway, but to train them and place them in jobs requiring significant skills. It is possible to design such a decentralized, on-the-job training program, but it requires intensive monitoring to insure that initially unskilled persons are trained for skilled positions. Applicants must be screened to assure that they are disadvantaged, training must be supervised, and payment to firms must depend at least, in part, on retention of the trainee in an acceptable job. This kind of scrutiny is expensive. It is also distasteful to firms and an incentive payment may have to be added so as not to discourage employer participation.

More significantly, although such a program may be quite successful in improving the status of individual trainees, their progress may come at the expense of others.[10] Theoretically, training unskilled persons adds to society's productive capacity. In situations of excess demand or in a competitive economy with flexible wages and prices, newly trained people can be absorbed easily. Where markets are not competitive and wages and prices are downwardly inflexible, displacement is a very real possibility. If the training subsidy is sufficient, employers can be induced to hire disadvantaged persons for existing (or expected) vacancies. But subsidies to cover unusual training expenses do not constitute permanent reductions in a firm's cost of production (as a wage subsidy does), and hence they do not expand employment. Similarly, they do not increase the demand for output. They merely increase the probability that firms will hire disadvantaged persons in preference to others.

The placement success of all training programs is quite sensitive to labor market conditions. JOBS is no exception. During the first 2 years of the program's operation unemployment rates were low (3.5–3.6 percent), and firms had difficulty filling vacancies. The increased contacts through JOBS between employers with vacancies and the Employment Service, WIN, and CEP may have increased employment among the disadvantaged during this period. But when unemployment rates began to rise in 1970, firms laid off workers and canceled JOBS contracts. Persons who had been placed through the JOBS program had little seniority and, hence, were among the first victims of the recession. In the context of generally contracting demand, training programs may merely improve the credentials of the unemployed.

Unless workers are retained in skilled positions following the completion of training, it is very difficult to verify that they were trained. As it stands, the JOBS program provides no incentive for retention of the worker. Firms are paid a subsidy only during the training period. This creates an opportunity for employers with high turnover rates among their low-skilled workers to subvert the program, and per-

[10] Unions are very aware of this. Greenleigh reported that NAB officials and employers complained about union resistance to the JOBS program, and that they had been relatively unsuccessful in obtaining union cooperation and participation, Greenleigh Associates, Inc., op. cit., p. 58.

functory monitoring seems to have made the opportunity all too tempting for some. The numerous instances of trainees holding unskilled jobs indicate that an elaborate game of musical chairs was played in which lower cost disadvantaged were substituted for other employees as these others vacated their jobs. The solution to this problem is to make partial payment of the training subsidy conditional upon the employee being retained in an acceptable job for a specified period of time following training.

SUPPLEMENTARY MATERIAL

I. Description of Manpower Programs

Job Corps

The Job Corps provides remedial education, training in job skills, and counseling services to disadvantaged [1] youths aged 16 to 21, of both sexes, who require a change in environment to become productive and employable. Training is designed to be completed in 9 months. There are now a few urban commuter facilities but most are live-in. Federally funded, the Job Corps is administered by the Manpower Administration (Department of Labor).

Programs are operated by private corporations, State or Federal agencies, universities or nonprofit organizations. Enrollees receive room and board, medical and dental care, clothing, and living allowances of $30 for the first months and up to $50 thereafter. An additional $50 per month of satisfactory service is payable on completion of training. If a trainee stipulates that a portion of his living allowance be used to support his wife and children, a matching grant of up to $25 per month is provided.

Enrollees must be citizens or permanent residents without serious criminal records, who have dropped out of school for at least 3 months, are underprivileged, and have been unable to find or hold adequate jobs. Trainees are placed in jobs, other training programs, the Armed Forces, secondary schools, and colleges.

Job Opportunities in the Business Sector

JOBS encourages and provides technical and monetary assistance to private industry for hiring, training, retraining and upgrading hard-core unemployed or underemployed persons over age 18. It is administered under a cooperative arrangement between the Manpower Administration and the National Alliance of Businessmen.

All private-sector companies located in the United States, whatever their size, are eligible for grants to offset the added costs of counseling, related education, job training, transportation, and the full range of supportive services needed. Contracts are for a maximum of 18 months although individual training periods for employees may not exceed 44 weeks.

Enrollees must be disadvantaged or subject to other special obstacles to employment.

There is both a contract and a noncontract (voluntary) component to this program. Under the former, private employers enter into negotiated contracts with the Department of Labor for employment and training of disadvantaged persons. Under the noncontract component, private employers pledge to hire specific numbers of disadvantaged persons without any cost reimbursement by the Government.

[1] The official definition of disadvantaged is "poor, and without suitable employment, and also either a school dropout, under 22 or over 45, or handicapped."

Manpower Development and Training Act (Institutional)

MDTA Institutional provides formal education and classroom training to unemployed and underemployed persons. After 1966 at least 65 percent of enrollees are supposed to have been disadvantaged. Federal grants are apportioned to States based on a formula which takes into consideration (among other things) employment opportunities available within it and the relative size of its labor force and its unemployment. The program is administered by the Manpower Administration and the Office of Education (Department of Health, Education, and Welfare). States pay up to 10 percent of the costs of training in cash or in kind.

Training is provided in skill centers or vocational schools; these may be either publicly or privately operated. Training courses are designed to be completed in less than a year, and are supposed to equip trainees with the skills necessary to fill local vacancies. Up to 20 weeks of training may be used for basic education and instruction in employment orientation. Training allowances are provided to household heads (or members of a household whose head is unemployed) who have had at least a year's experience in gainful employment. Youth allowances are paid to disadvantaged persons aged 17 to 21.

Manpower Development and Training Act (On-the-Job)

MDTA–OJT provided instruction plus supervised work at the job site for unemployed and underemployed persons aged 16 and over and workers whose jobs were endangered by changing technology. MDTA–OJT was phased out in January of 1971 and is now called JOP (Jobs Optional Program), and is run by the States through their Employment Services with Federal funding.

Trainees are hired by employers and trained on-site for specific jobs. Supplementary classroom instruction is sometimes given. Contracts are negotiated with employers (public and private) who receive subsidies for approved trainees to cover salary of instructors, materials, damaged or spoiled production material, and rented equipment or space if needed. The employers make the final decision on whether to hire trainees referred by the Employment Service. Enrollees must be individuals who cannot reasonably be expected to secure appropriate full-time employment without training.

Neighborhood Youth Corps

NYC operates three programs. The out-of-school program provides work experience, training, counseling and remedial education for youths from low-income families who have dropped out of school in order to enable them to return to school if possible or else to acquire skills to improve employability. The in-school and summer programs provide earning opportunities to students from low-income families to enable them to remain in school while receiving work experience. Sixty percent of enrollees were urban in 1971.

NYC is administered by the Manpower Administration and the Federal Government pays up to 90 percent of the cost of projects, with local sponsors making up the rest in cash or kind. Within each community, sponsors may be public or private agencies or companies which operate skill-training programs for employees.

Enrollees for the in-school and summer programs are from grades 9 to 12. They work a maximum of 15 hours a week during the school year or nine 26-hour weeks during the summer. Enrollees in the out-of-school program are unemployed or underemployed school dropouts aged 16 to 17. Maximum participation is 40 hours per week including counseling and remedial education.

NYC is a recent though not significantly changed version of the depression era National Youth Administration (NYA). NYA lasted from 1935 to 1943, came in both in-school and out-of-school editions, and was phased out during the war.

Work Incentive Program

WIN provides training to recipients of AFDC (Aid to Families with Dependent Children). On-the-job training, counseling, and placement are provided for those ready for employment. Basic education, work orientation, skill training, work experience, and counseling are provided to improve the employability of persons not ready for employment. Placement in public service employment (formerly special work projects), arranged by agreement with public or private nonprofit organizations, is provided for individuals not ready for employability training. These services are supplemented by those of State welfare agencies, including full reimbursement for day care and work expenses. Participants' monthly earnings are taxed at a rate of two-thirds for all earnings above the first $30. Formerly earnings of AFDC recipients were taxed at 100 percent. The reduced tax rates are not extended to those WIN enrollees who are placed in public service employment.

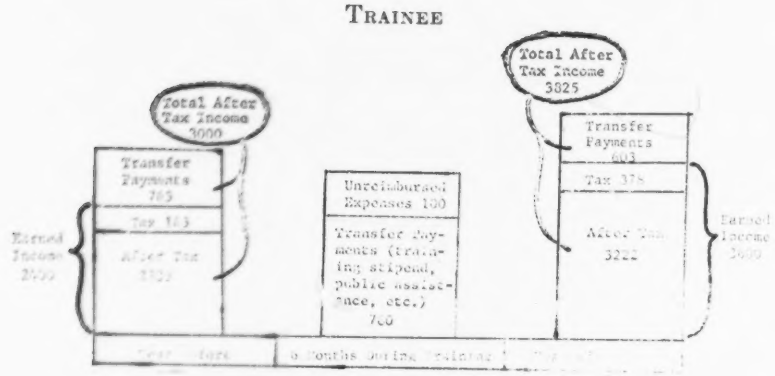
WIN is administered by the Manpower Administration, and operated at the local level by State Employment Service offices. Federal funding has recently been increased from 80 to 90 percent, with States providing the rest in cash or kind.

II. Illustrative Example on Benefits, Costs, and Investment Criteria

The following hypothetical example traces the economic situation of a trainee and his untrained twin (control group counterpart) from the year before to the year after training.[2] Benefits and costs are calculated from three points of view (social, private, and government), and the economic efficiency of the training is determined using each of the three investment criteria discussed in the text (the benefit-cost ratio, the net present value, and the internal rate of return). The basic data used throughout the example are presented in figure 1.

[2] This example is a slightly modified version of one developed by Joe N. Nay, John W. Scanlon, and Joseph S. Wholey, Benefits and Costs of Manpower Training Programs: A Synthesis of Previous Studies with Reservations and Recommendations, The Urban Institute, Washington, D.C., June 30, 1971, appendices A and B.

TRAINEE


Total After Tax Income 3825
Transfer Payments 603
Tax 378
Earned Income 3600
Unreimbursed Expenses 100
Transfer Payments (training stipend, public assistance, etc.) 700

UNTRAINED TWIN

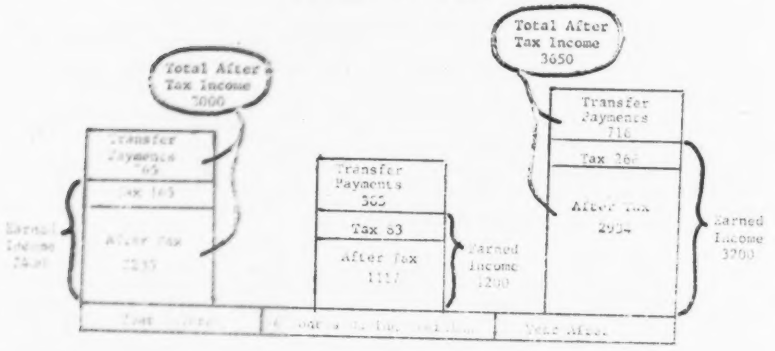


FIGURE 1

Social Economic Viewpoint

From society's point of view the cost of training is the value of the output which could have been produced with the resources actually employed in training. The value of the output which was foregone is measured by the cost of instructional and administrative resources, the unreimbursed expenses of the enrollee over and above any expenses which he would have incurred had he been working, and the foregone earnings of the enrollee. These amount to $2,100 and are depicted in figure 2. Note that the foregone earnings of the enrollee are assumed to be equal to what the untrained twin earned during the training period.

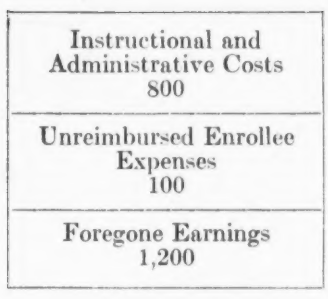


FIGURE 2

In the first year after training the trainee earned $400 more than his untrained twin. Optimally, we would like to know how much more the trainee earned than the twin throughout the remainder of their lifetimes. Since the observation period includes only one posttraining year, we are forced to make an assumption about the duration and future size of the training benefits. The Office of Management and Budget projects the benefits of social programs for 10 years and discounts them at a rate of 10 percent per year. Under this assumption the discounted or present value of the benefits is ($400) (6.14) = $2,576.[3] The calculated value for each of the investment criteria appears in the table below. From the social point of view the training in this example was economically efficient.

	Value needed for economic efficiency	*Social viewpoint*	*Trainee viewpoint*	*Government viewpoint*
Present value of benefits (B)		$2, 576	$1, 074	$1, 382
Costs (C)		$2, 100	$820	$1, 380
Benefit-cost ratio (B/C)	B/C>1	1. 23	1. 31	1. 001
Net present value (B–C)	B–C>0	$476	$254	$2
Internal rate of return (i)	i>. 10	. 13	. 16	. 10025

Private or Trainee Viewpoint

The trainee's cost of participating in the program is the reduction in his after tax income. The twin's after tax income during the training period was $1,117 + $383 = $1,500, while the trainee had only $780 − $100 = $680. Hence, by participating in training, he gave up $1,500 − $680 = $820.

The annual benefit to the trainee is the increase in his posttraining after tax income ($175 = $3,825 − $3,650). If benefits last 10 years and the discount rate is 10 percent, the present value of the benefits is ($175) (6.14) = $1,074. The entries in figure 3 show that training was worthwhile from the trainee's point of view.

[3] When the rate of interest is 10 percent, $6.14 is the present value of a 10 year annuity of $1. In other words, one would have to pay $6.14 now for an asset which guaranteed to provide the owner with $1 in each of the next 10 years.

Government Viewpoint

The cost to the Government is the net loss to the Treasury during the training period. The twin received $300 from the Government during this period ($383 in transfer payments less $83 in taxes). The Government had administrative and instructional expenses of $900 and made $780 worth of transfer payments to the trainee. Thus the net outflow from the Treasury was $1,680 − $300 = $1,380.

The annual benefit to the Government is the net posttraining gain to the Treasury. In the first year after training the twin received $450 from the Government ($716 in transfer payments less $266 in taxes), while the trainee received only $225 ($603 in transfer payments less $378 in taxes). The annual net gain to the Treasury is $450 − $225 = $225. If benefits last 10 years and the discount rate is 10 percent, the present value of the benefits is ($225) (6.14) = $1,382. As figure 3 shows, the project was economically efficient from the government's point of view.

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